PREFACE.

A THIRD series of Essays, in great measure corresponding with this, was issued a year and a half since in the United States; but a reason then in force deterred me from issuing it here. That reason no longer holding, I have prepared this volume, differing from its counterpart by some omissions and some additions. One essay added to the American Edition, I exclude because a volume of Essays already published here, contains it; and another, on "Morals and Moral Sentiments," which first appeared in the *Fortnightly Review* for April, 1871, I exclude because I have embodied the essential parts of it in certain closing chapters of the *Principles of Psychology*, and I have no wish to perpetuate its controversial and personal parts.

One of the included essays—that entitled "Specialized Administration"—though in a measure controversial, I reproduce; because it contains facts and arguments which are, I think, of more than ephemeral interest, and because permanence having been given to the question it deals with, the reply may fitly have a permanent form. Not only do I reciprocate most cordially the utterance of kind feeling with which Prof. Huxley prefaces the republication of his "Administrative Nihilism;" and not

only do I rejoice that divergence of opinion is not likely to weaken a long-established, and highly-valued friendship; but I may add the expression of my great regret that I should be obliged still to differ from one whose agreement, more than that of any other friend, confirms me in any conclusion I have drawn. I fear, however, that, holding though we do so many beliefs in common, there remains respecting this sociological question no alternative but amicable opposition.

On the brief rejoinder to my arguments which Prof. Huxley makes in the preface to his *Critiques and Addresses*, I may here say a few words. The reasons he gives for still thinking that the name "Administrative Nihilism" fitly indicates the system which I have described as "negatively regulative," are, I think, adequately met by asking whether "Ethical Nihilism" would fitly describe the remnant of the decalogue, were all its positive injunctions omitted. If the eight commandments which, substantially or literally, come under the form "thou shalt not," constitute by themselves a set of rules which can scarcely be called nihilistic; I do not see how an administrative system limited to the enforcement of such rules can be called nihilistic: especially if to the punishment of murder, adultery, stealing, and false-witness, it adds the punishment of assault, breach of contract, and all minor aggressions, down to the annoyance of neighbours by nuisances. Respecting the second and essential question, whether limitation of the internal functions of government to those which are negatively regulative, is consistent with that theory of the social organism and its controlling

agencies held by me, I may say that the insufficiency of my reply has not, I think, been shown. I was tacitly asked how the analogy I have drawn between those governmental structures by which the parts of the body politic have their actions regulated and those nervous structures which regulate the organic actions of the individual living body, is to be reconciled with my belief that social activities will in the main adjust themselves. My answer was this. I recognized as essential the positively-regulative functions of the State in respect to the offensive and defensive appliances needful for national self-preservation, during the predatory phase of social evolution; and I not only admitted the importance of its negatively-regulative functions in respect to the internal social activities, but insisted that these should be carried out much more efficiently than now. Assuming always, however, that the internal social activities continue subject to that restraining action of the State which consists in preventing aggressions, direct and indirect, I contended that the co-ordination of these internal social activities is effected by other structures of a different kind. I aimed to show that my two beliefs are not inconsistent, by pointing out that in the individual organism, also, those vital activities which parallel the activities constituting national life, are regulated by a substantially-independent nervous system. Prof. Huxley does, indeed, remind me that recent researches show increasingly the influence of the cerebro-spinal nervous system over the processes of organic life; against which, however, has to be set the growing evidence of

CONTENTS.

PREFACE

TO THE THIRD EDITION.

In the preface to the second edition, I have described myself as resisting the temptation to amplify, which the occasion raised. Reasons have since arisen for yielding to the desire which I then felt to add justifications of the scheme set forth.

The immediate cause for this change of resolve, has been the publication of several objections by Prof. Bain in his Logic. Permanently embodied, as these objections are, in a work intended for the use of students, they demand more attention than such as have been made in the course of ordinary criticism; since, if they remain unanswered, their prejudicial effects will be more continuous.

While to dispose of these I seize the opportunity afforded by a break in my ordinary work, I have thought it well at the same time to strengthen my own argument, by a re-statement from a changed point of view.

Feb., 1871.

PREFACE

TO THE SECOND EDITION.

The first edition of this Essay is not yet out of print. But a proposal to translate it into French having been made by Professor Réthoré, I have decided to prepare a new edition free from the imperfections which criticism and further thought have disclosed, rather than allow these imperfections to be reproduced.

The occasion has almost tempted me into some amplification. Further arguments against the classification of M. Comte, and further arguments in support of the classification here set forth, have pleaded for utterance. But reconsideration has convinced me that it is both needless and useless to say more —needless because those who are not committed will think the case sufficiently strong as it stands, and useless because to those who are committed additional reasons will seem as inadequate as the original ones.

This last conclusion is thrust on me by seeing how little M. Littré, the leading expositor of M. Comte, is influenced by fundamental objections the force of which he admits. After quoting one of these, he

says, with a candour equally rare and admirable, that he has vainly searched M. Comte's works and his own mind for an answer. Nevertheless, he adds—"j'ai réussi, je crois, à écarter l'attaque de M. Herbert Spencer, et à sauver le fond par des sacrifices indispensables mais accessoires." The sacrifices are these. He abandons M. Comte's division of Inorganic Science into Celestial Physics and Terrestrial Physics—a division which, in M. Comte's scheme, takes precedence of all the rest; and he admits that neither logically nor historically does Astronomy come before Physics, as M. Comte alleges. After making these sacrifices, which most will think too lightly described as "sacrifices indispensables mais accessoires," M. Littré proceeds to rehabilitate the Comtean classification in a way which he considers satisfactory, but which I do not understand. In short, the proof of these incongruities affects his faith in the Positivist theory of the sciences, no more than the faith of a Christian is affected by proof that the Gospels contradict one another.

Here in England I have seen no attempt to meet the criticisms with which M. Littré thus deals. There has been no reply to the allegation, based on examples, that the several sciences do not develop in the order of their decreasing generality; nor to the allegation, based on M. Comte's own admissions, that within each science the progress is not, as he says it is, from the general to the special; nor to

the allegation that the seeming historical precedence of Astronomy over Physics in M. Comte's pages, is based on a verbal ambiguity—a mere sleight of words; nor to the allegation, abundantly illustrated, that a progression in an order the reverse of that asserted by M. Comte may be as well substantiated; nor to various minor allegations equally irreconcileable with his scheme. I have met with nothing more than iteration of the statement that the sciences *do* conform, logically and historically, to the order in which M. Comte places them; regardless of the assigned evidence that they *do not.*

Under these circumstances it is unnecessary for me to say more; and I think I am warranted in continuing to hold that the Comtean classification of the sciences is demonstrably untenable.

While, however, I have not entered further into the controversy, as I thought of doing, I have added at the close an already-published discussion, no longer easily accessible, which indirectly enforces the general argument.

LONDON, 23RD APRIL, 1869.

THE

CLASSIFICATION OF THE SCIENCES.

In an essay on "The Genesis of Science," originally published in 1854, I endeavoured to show that the Sciences cannot be rationally arranged in serial order. Proof was given that neither the succession in which the Sciences are placed by M. Comte (to a criticism of whose scheme the essay was in part devoted), nor any other succession in which the Sciences can be placed, represents either their logical dependence or their historical dependence. To the question—How may their relations be rightly expressed? I did not then attempt any answer. This question I propose now to consider.

A true classification includes in each class, those objects which have more characteristics in common with one another, than any of them have in common with any objects excluded from the class. Further, the characteristics possessed in common by the colligated objects, and not possessed by other objects, are more radical than any characteristics possessed in common with other objects—involve more numerous

dependent characteristics. These are two sides of the same definition. For things possessing the greatest number of attributes in common, are things that possess in common those essential attributes on which the rest depend; and, conversely, the possession in common of the essential attributes, implies the possession in common of the greatest number of attributes. Hence, either test may be used as convenience dictates.

If, then, the Sciences admit of classification at all, it must be by grouping together the like and separating the unlike, as thus defined. Let us proceed to do this.

The broadest natural division among the Sciences, is the division between those which deal with the abstract relations under which phenomena are presented to us, and those which deal with the phenomena themselves. Relations of whatever orders, are nearer akin to one another than they are to any objects. Objects of whatever orders, are nearer akin to one another than they are to any relations. Whether, as some hold, Space and Time are forms of Thought; or whether, as I hold myself, they are forms of Things, that have become forms of Thought through organized and inherited experience of Things; it is equally true that Space and Time are contrasted absolutely with the existences disclosed to us in Space and Time; and that the Sciences which deal exclusively with Space and Time, are separated by the profoundest of all distinctions from the Sciences which deal with the

existences that Space and Time contain. Space is the abstract of all relations of co-existence. Time is the abstract of all relations of sequence. And dealing as they do entirely with relations of co-existence and sequence, in their general or special forms, Logic and Mathematics form a class of the Sciences more widely unlike the rest, than any of the rest can be from one another.

The Sciences which deal with existences themselves, instead of the blank forms in which existences are presented to us, admit of a sub-division less profound than the division above made, but more profound than any of the divisions among the Sciences individually considered. They fall into two classes, having quite different aspects, aims, and methods. Every phenomenon is more or less composite—is a manifestation of force under several distinct modes. Hence result two objects of inquiry. We may study the component modes of force separately; or we may study them in their relations, as co-operative factors in this composite phenomenon. On the one hand, neglecting all the incidents of particular cases, we may aim to educe the laws of each mode of force, when it is uninterfered with. On the other hand, the incidents of the particular case being given, we may seek to interpret the entire phenomenon, as a product of the several forces simultaneously in action. The truths reached through the first kind of inquiry, though concrete inasmuch as they have actual existences for their subject-matters,

are abstract inasmuch as they refer to the modes of existence apart from one another; while the truths reached by the second kind of inquiry are properly concrete, inasmuch as they formulate the facts in their combined order, as they occur in Nature.

The Sciences, then, in their main divisions, stand thus:—

SCIENCE is	that which treats of the forms in which phenomena are known to us		ABSTRACT SCIENCE	Logic and Mathematics.
	that which treats of the phenomena themselves	in their elements	ABSTRACT-CONCRETE SCIENCE	Mechanics, Physics, Chemistry, etc.
		in their totalities	CONCRETE SCIENCE	Astronomy, Geology, Biology, Psychology, Sociology, etc.

It is needful to define the words *abstract* and *concrete* as thus used; since they are sometimes used with other meanings. M. Comte divides Science into abstract and concrete; but the divisions which he distinguishes by these names are quite unlike those above made. Instead of regarding some Sciences as wholly abstract, and others as wholly concrete, he regards each Science as having an abstract part, and a concrete part. There is, according to him, an abstract mathematics and a concrete mathematics—an

abstract biology and concrete biology. He says:—"Il faut distinguer, par rapport à tous les ordres de phénomènes, deux genres de sciences naturelles: les unes abstraites, générales, ont pour objet la découverte des lois qui régissent les diverses classes de phénomènes, en considérant tous les cas qu'on peut concevior; les autres concrètes, particulières, descriptives, et qu'on désigne quelquefois sous le nom de sciences naturelles proprement dites, consistent dans l'application de ces lois a l'histoire effective de différens êtres existans." And to illustrate the distinction, he names general physiology as abstract, and zoology and botany as concrete. Here it is manifest that the words *abstract* and *general* are used as synonymous. They have, however, different meanings; and confusion results from not distinguishing their meanings. Abstractness means *detachment from* the incidents of particular cases. Generality means *manifestation in* numerous cases. On the one hand, the essential nature of some phenomenon is considered, apart from disguising phenomena. On the other hand, the frequency of the phenomenon, with or without disguising phenomena, is the thing considered. Among the ideal relations of numbers the two coincide; but excluding these, an abstract truth is not realizable to perception in any case of which it is asserted, whereas a general truth is realizable to perception in every case of which it is asserted. Some illustrations will make the distinction clear. Thus it is an abstract truth that the angle contained

in a semi-circle is a right angle—abstract in the sense that though it does not hold in actually-constructed semi-circles and angles, which are always inexact, it holds in the ideal semi-circles and angles abstracted from real ones; but this is not a general truth, either in the sense that it is commonly manifested in Nature, or in the sense that it is a space-relation that comprehends many minor space-relations: it is a quite special space-relation. Again, that the momentum of a body causes it to move in a straight line at a uniform velocity, is an abstract-concrete truth—a truth abstracted from certain experiences of concrete phenomena; but it is by no means a general truth: so little generality has it, that no one fact in Nature displays it. Conversely, surrounding things supply us with hosts of general truths that are not in the least abstract. It is a general truth that the planets go round the Sun from West to East—a truth which holds good in something like a hundred cases (including the cases of the planetoids); but this truth is not at all abstract, since it is perfectly realized as a concrete fact in every one of these cases. Every vertebrate animal whatever, has a double nervous system; all birds and all mammals are warm-blooded—these are general truths, but they are concrete truths: that is to say, every vertebrate animal individually presents an entire and unqualified manifestation of this duality of the nervous system; every living bird exemplifies absolutely or completely

the warm-bloodedness of birds. What we here call, and rightly call, a general truth, is simply a proposition which *sums up* a number of our actual experiences; and not the expression of a truth *drawn from* our actual experiences, but never presented to us in any of them. In other words, a general truth colligates a number of particular truths; while an abstract truth colligates no particular truths, but formulates a truth which certain phenomena all involve, though it is actually seen in none of them.

Limiting the words to their proper meanings as thus defined, it becomes manifest that the three classes of Sciences above separated, are not distinguishable at all by differences in their degrees of generality. They are all equally general; or rather they are all, considered as groups, universal. Every object whatever presents at once the subject-matter for each of them. In the smallest particle of substance we have simultaneously illustrated the abstract truths of relation in Time and Space; the abstract-concrete truths in conformity with which the particle manifests its several modes of force; and the concrete truths which are the laws of the joint manifestation of these modes of force. Thus these three classes of Sciences severally formulate different, but co-extensive, classes of facts. Within each group there are truths of greater and less generality: there are general abstract truths, and special abstract truths; general abstract-concrete truths, and special abstract-concrete truths;

general concrete truths, and special concrete truths. But while within each class there are groups and sub-groups and sub-sub-groups which differ in their degrees of generality, the classes themselves differ only in their degrees of abstractness.*

* Some propositions laid down by M. Littré, in his lately-published book—*Auguste Comte et la Philosophie Positive*, may fitly be dealt with here. In the candid and courteous reply he makes to my strictures on the Comtean classification in "The Genesis of Science," he endeavours to clear up some of the inconsistencies I pointed out; and he does this by drawing a distinction between objective generality and subjective generality. He says—"qu'il existe deux ordres de généralité, l'une objective et dans les choses, l'autre subjective, abstraite et dans l'esprit." This sentence, in which M. Littré makes subjective generality synonymous with abstractness, led me at first to conclude that he had in view the same distinction as that which I have above explained between generality and abstractness. On re-reading the paragraph, however, I found this was not the case. In a previous sentence he says—"La biologie a passé de la considération des organes à celles des tissus, plus généraux que les organes, et de la considération des tissus à celle des éléments anatomiques, plus généraux que les tissus. Mais cette généralité croissante est subjective non objective, abstraite non concrète." Here it is manifest that abstract and concrete, are used in senses analogous to those in which they are used by M. Comte; who, as we have seen, regards general physiology as abstract and zoology and botany as concrete. And it is further manifest that the word abstract, as thus used, is not used in its proper sense. For, as above shown, no such facts as those of anatomical structure can be abstract facts; but can only be more or less general facts. Nor do I understand M Littré's point of view when he regards these more general facts of anatomical structure, as *subjectively* general and not *objectively* general. The structural phenomena presented by any tissue, such as mucous membrane, are more general than the phenomena presented by any of the organs which mucous membrane goes to form, simply in the sense that the phenomena peculiar to the membrane are repeated in a greater number of instances than the phenomena peculiar to any organ into the composition of which the membrane enters. And, similarly, such facts as have been established respecting the anatomical elements of tissues, are more general than the facts established respecting any particular tissue, in the sense that they are facts which organic bodies exhibit in a greater number of cases—they are *objectively* more general; and they can be called *subjectively* more general only in the sense that the conception corresponds with the phenomena.

Let me endeavour to clear up this point:—There is, as M. Littré truly says, a decreasing generality that is objective. If we omit the phenomena of Dissolution, which are changes from the special to the general, all changes which matter undergoes are from the general to the special—are changes involving a decreasing

Passing to the sub-divisions of these classes, we find that the first class is separable into two parts—the one containing universal truths, the other non-universal truths. Dealing wholly with relations apart from related things, Abstract Science considers first, that which is common to all relations whatever; and second, that which is common to each order of relations. Besides the indefinite and variable connexions which exist among phenomena, as occurring together in Space and Time, we find that there are also definite

generality in the united groups of attributes. This is the progress of *things*. The progress of *thought*, is not only in the same direction, but also in the opposite direction. The investigation of Nature discloses an increasing number of specialities; but it simultaneously discloses more and more the generalities within which these specialities fall. Take a case. Zoology, while it goes on multiplying the number of its species, and getting a more complete knowledge of each species (decreasing generality); also goes on discovering the common characters by which species are united into larger groups (increasing generality). Both these are subjective processes; and in this case, both orders of truths reached are concrete—formulate the phenomena as actually manifested.

M. Littré, recognizing the necessity for some modification of the hierarchy of the Sciences, as enunciated by M. Comte, still regards it as substantially true; and for proof of its validity, he appeals mainly to the essential *constitutions* of the Sciences. It is unnecessary for me here to meet, in detail, the arguments by which he supports the proposition, that the essential constitutions of the Sciences, justify the order in which M. Comte places them. It will suffice to refer to the foregoing pages, and to the pages which are to follow, as containing the definitions of those fundamental characteristics which demand the grouping of the Sciences in the way pointed out. As already shown, and as will be shown still more clearly by and bye, the radical differences of constitution among the Sciences, necessitate the colligation of them into the three classes—Abstract, Abstract-Concrete, and Concrete. How irreconcilable is M. Comte's classification with these groups, will be at once apparent on inspection. It stands thus:—

Mathematics (including rational Mechanics), ...	partly Abstract, partly Abstract-Concrete.
Astronomy	Concrete.
Physics..	Abstract-Concrete.
Chemistry	Abstract-Concrete.
Biology...	Concrete.
Sociology	Concrete.

and invariable connexions—that between each kind of phenomenon and certain other kinds of phenomena, there exist uniform relations. This is a universal abstract truth—that there is an unchanging order among things in Space and Time. We come next to the several kinds of unchanging order, which, taken together, form the subjects of the second division of Abstract Science. Of this second division, the most general sub-division is that which deals with the natures of the connexions in Space and Time, irrespective of the terms connected. The conditions under which we may predicate a relation of coincidence or proximity in Space and Time (or of non-coincidence or non-proximity) form the subject-matter of Logic. Here the natures and amounts of the terms between which the relations are asserted (or denied) are of no moment: the propositions of Logic are independent of any qualitative or quantitative specification of the related things. The other sub-division has for its subject-matter, the relations between terms which are specified quantitatively but not qualitatively. The amounts of the related terms, irrespective of their natures, are here dealt with; and Mathematics is a statement of the laws of quantity considered apart from reality. Quantity considered apart from reality, is occupancy of Space or Time; and occupancy of Space or Time is measured by the number of coexistent or sequent positions occupied. That is to say, quantities can be

compared and the relations between them established, only by some direct or indirect enumeration of their component units; and the ultimate units into which all others are decomposable, are such occupied positions in Space as can, by making impressions on consciousness, produce occupied positions in Time. Among units that are unspecified in their natures (extensive, protensive, or intensive), but are ideally endowed with existence considered apart from attributes, the quantitative relations that arise, are those most general relations expressed by numbers. Such relations fall into either of two orders, according as the units are considered simply as capable of filling separate places in consciousness, or according as they are considered as filling places that are not only separate, but equal. In the one case, we have that indefinite calculus by which numbers of abstract existences, but not sums of abstract existence, are predicable. In the other case, we have that definite calculus by which both numbers of abstract existences and sums of abstract existence are predicable. Next comes that division of Mathematics which deals with the quantitative relations of magnitudes (or aggregates of units) considered as coexistent, or as occupying Space—the division called Geometry. And then we arrive at relations, the terms of which include both quantities of Time and quantities of Space—those in which times are estimated by the units of space traversed at a uniform velocity, and those in which equal

units of time being given, the spaces traversed with uniform or variable velocities are estimated. These Abstract Sciences, which are concerned exclusively with relations and with the relations of relations, may be grouped as shown in Table I.

Passing from the Sciences that treat of the ideal or unoccupied forms of relations, and turning to the Sciences that treat of real relations, or the relations among realities, we come first to those Sciences which deal with realities, not as they are habitually manifested to us, but with realities as manifested in their different modes, when these are artificially separated from one another. In the same way that the Abstract Sciences are ideal, relatively to the Abstract-Concrete and Concrete Sciences; so the Abstract-Concrete Sciences are ideal, relatively to the Concrete Sciences. Just as Logic and Mathematics have for their object to generalize the laws of relation, qualitative and quantitative, apart from related things; so, Mechanics, Physics, Chemistry, etc., have for their object to generalize the laws of relation which different modes of Matter and Motion conform to, when severally disentangled from those actual phenomena in which they are mutually modified. Just as the geometrician formulates the properties of lines and surfaces, independently of the irregularities and thicknesses of lines and surfaces as they really exist; so the physicist and the chemist formulate the mani-

TABLE I.

ABSTRACT SCIENCE.

- Universal law of relation—an expression of the truth that uniformities of connexion obtain among modes of Being, irrespective of any specification of the natures of the uniformities of connexion.
- Laws of relations
  - that are qualitative; or that are specified in their natures as relations of coincidence or proximity in Time and Space, but not necessarily in their terms: the natures and amount of which are indifferent. (LOGIC.)*
  - that are quantitative (MATHEMATICS)
    - negatively: the terms of the relations being definitely-related sets of positions in space; and the facts predicated being the absences of certain quantities. (*Geometry of Position.***)
    - positively: the terms being magnitudes composed of
      - units that are equal only as having independent existences. (*Indefinite Calculus.*†)
      - equal units
        - the equality of which is not defined as extensive, protensive, or intensive (*Definite Calculus*)
          - when their numbers are completely specified. (*Arithmetic.*)
          - when their numbers are specified only
            - in their relations. (*Algebra.*)
            - in the relations of their relations. (*Calculus of Operations.*)
        - the equality of which is that of extension
          - considered in their relations of coexistence. (*Geometry.*)
          - considered as traversed in Time
            - that is wholly indefinite. (*Kinematics.*)
            - that is divided into equal units. (*Geometry of Motion.*‡)

* This definition includes the laws of relations called necessary, but not those of relations called contingent. These last, in which the probability of an inferred connexion varies with the number of times such connexion has occurred in experience, are rightly dealt with mathematically.

** Here, by way of explanation of the term negatively-quantitative, it will suffice to instance the proposition that certain three lines will meet in a point, as a negatively-quantitative proposition; since it asserts the absence of any quantity of space between their intersections. Similarly, the assertion that certain three points will always fall in a straight line, is negatively-quantitative; since the conception of a straight line implies the negation of any lateral quantity, or deviation.

† Lest the meaning of this division should not be understood, it may be well to name, in illustration, the estimates of the statistician. Calculations respecting population, crime, disease, etc., have results which are correct only numerically, and not in respect of the totalities of being or action represented by the numbers.

‡ Perhaps it will be asked—How can there be a Geometry of Motion into which the conception of Force does not enter? The reply is, that the time-relations and space-relations of Motion may be considered apart from those of Force, in the same way that the space-relations of Matter may be considered apart from Matter.

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festations of each mode of force, independently of the disturbances in its manifestations which other modes of force cause in every actual case. In works on Mechanics, the laws of motion are expressed without reference to friction and resistance of the medium. Not what motion ever really is, but what it would be if retarding forces were absent, is asserted. If any retarding force is taken into account, then the effect of this retarding force is alone contemplated: neglecting the other retarding forces. Consider, again, the generalizations of the physicist respecting molecular motion. The law that light varies inversely as the square of the distance, is absolutely true only when the radiation goes on from a point without dimensions, which it never does; and it also assumes that the rays are perfectly straight, which they cannot be unless the medium differs from all actual media in being perfectly homogeneous. If the disturbing effects of changes of media are investigated, the formulæ expressing the refractions take for granted that the new media entered are homogeneous; which they never really are. Even when a compound disturbance is allowed for, as when the refraction undergone by light in traversing a medium of increasing density, like the atmosphere, is calculated, the calculation still supposes conditions that are unnaturally simple—it supposes that the atmosphere is not pervaded by heterogeneous currents, which it always is. Similarly with the inquiries of the

chemist. He does not take his substances as Nature supplies them. Before he proceeds to specify their respective properties, he purifies them—separates from each all trace of every other. Before ascertaining the specific gravity of a gas, he has to free this gas from the vapour of water, usually mixed with it. Before describing the properties of a salt, he guards against any error that may arise from the presence of an uncombined portion of the acid or base. And when he alleges of any element that it has a certain atomic weight, and unites with such and such equivalents of other elements, he does not mean that the results thus expressed are exactly the results of any one experiment; but that they are the results which, after averaging many trials, he concludes would be realized if absolute purity could be obtained, and if the experiments could be conducted without loss. His problem is to ascertain the laws of combination of molecules, not as they are actually displayed, but as they would be displayed in the absence of those minute interferences which cannot be altogether avoided. Thus all these Abstract-Concrete Sciences have for their object, *analytical interpretation.* In every case it is the aim to decompose the phenomenon, and formulate its components apart from one another; or some two or three apart from the rest. Wherever, throughout these Sciences, synthesis is employed, it is for the verification of analysis.*

* I am indebted to Prof. Frankland for reminding me of an objection that may be

The truths elaborated are severally asserted, not as truths exhibited by this or that particular object; but as truths universally holding of Matter and Motion in their more general or more special forms, considered apart from particular objects, and particular places in space.

The sub-divisions of this group of Sciences, may be drawn on the same principle as that on which the sub-divisions of the preceding group were drawn. Phenomena, considered as more or less involved manifestations of force, yield on analysis, certain laws of manifestation that are universal, and other laws of manifestation, which, being dependent on conditions, are not universal. Hence the Abstract-Concrete Sciences are primarily divisible into—the laws of force considered apart from its separate modes, and laws of force considered under each of its separate modes. And this second division of the Abstract-Concrete group, is sub-divisible after a manner essentially analogous. It is needless to occupy space by

made to this statement. The production of new compounds by synthesis, has of late become an important branch of chemistry. According to certain known laws of composition, complex substances, which never before existed, are formed, and fulfil anticipations both as to their general properties and as to the proportions of their constituents—as proved by analysis. Here it may be said with truth, that analysis is used to verify synthesis. Nevertheless, the exception to the above statement is apparent only—not real. In so far as the production of new compounds is carried on merely for the obtainment of such new compounds, it is not Science but Art—the application of pre-established knowledge to the achievement of ends. The proceeding is a part of Science, only in so far as it is a means to the better interpretation of the order of Nature. And how does it aid the interpretation? It does it only by verifying the pre-established conclusions respecting the laws of molecular combination; or by serving further to explain them. That is to say, these syntheses, considered on their scientific side, have simply the purpose of *forwarding the analysis of the laws of chemical combination.*

defining these several orders and genera of Sciences. Table II. will sufficiently explain their relations.

We come now to the third great group. We have done with the Sciences which are concerned only with the blank forms of relations under which Being is manifested to us. We have left behind the Sciences which, dealing with Being under its universal mode, and its several non-universal modes regarded as independent, treats the terms of its relations as simple and homogeneous, which they never are in Nature. There remain the Sciences which, taking these modes of Being as they are connected with one another, have for the terms of their relations, those heterogeneous combinations of forces that constitute actual phenomena. The subject-matter of these Concrete-Sciences is the real, as contrasted with the wholly or partially ideal. It is their aim, not to separate and generalize apart the components of all phenomena; but to explain each phenomenon as a product of these components. Their relations are not, like those of the simplest Abstract-Concrete Sciences, relations between one antecedent and one consequent; nor are they, like those of the more involved Abstract-Concrete Sciences, relations between some few antecedents cut off in imagination from all others, and some few consequents similarly cut off; but they are relations each of which has for its terms a complete plexus of antecedents and a complete plexus of consequents. This is manifest in the

TABLE II.

ABSTRACT-CONCRETE SCIENCE.

- Universal laws of forces (tensions and pressures), as deducible from the persistence of force: the theorems of resolution and composition of forces.
- Laws of forces as manifested by matter
  - in masses (MECHANICS)
    - that are in equilibrium relatively to other masses
      - and are solid. (*Statics.*)
      - and are fluid. (*Hydrostatics.*)
    - that are not in equilibrium relatively to other masses
      - and are solid. (*Dynamics.*)
      - and are fluid. (*Hydrodynamics.*)
  - in molecules (MOLECULAR MECHANICS)
    - when in equilibrium: (*Molecular Statics*)
      - giving statical properties of matter
        - general, as impenetrability or space-occupancy.
        - special, as the forms resulting from molecular equilibrium.
      - giving statico-dynamical properties of matter (cohesion, elasticity, etc.)
        - when solid.
        - when liquid.
        - when gaseous.
    - when not in equilibrium: (*Molecular Dynamics*)
      - as resulting in a changed distribution of molecules
        - which alters their relative positions homogeneously
          - causing increase of volume (expansion, liquefaction, evaporation).
          - causing decrease of volume (condensation, solidification, contraction).
        - which alters their relative positions heterogeneously (*Chemistry*)
          - producing new relations of molecules (new compounds).
          - producing new relations of forces (new affinities).
      - as resulting in a changed distribution of molecular motion,
        - which, by integration, generates sensible motion.
        - which, by disintegration, generates insensible motion, under the forms of
          - *Heat.*
          - *Light.*
          - *Electricity.*
          - *Magnetism.*

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least involved Concrete Sciences. The astronomer seeks to explain the Solar System. He does not stop short after generalizing the laws of planetary movement, such as planetary movement would be did only a single planet exist; but he solves this abstract-concrete problem, as a step towards solving the concrete problem of the planetary movements as affecting one another. In astronomical language, "the theory of the Moon" means an interpretation of the Moon's motions, not as determined simply by centripetal and centrifugal forces, but as perpetually modified by gravitation towards the Earth's equatorial protuberance, towards the Sun, and even towards Venus—forces daily varying in their amounts and combinations. Nor does the astronomer leave off when he has calculated what will be the position of a given body at a given time, allowing for all perturbing influences; but he goes on to consider the effects produced by re-actions on the perturbing masses. And he further goes on to consider how these mutual perturbations of the planets cause, during a long period, increasing deviations from a mean state; and then how compensating perturbations cause continuous decrease in the deviations. That is, the goal towards which he ever strives, is a complete explanation of these complex planetary motions in their totality. Similarly with the geologist. He does not take for his problem only those irregularities of the Earth's crust that are worked by denudation; or only those which igneous

action causes. He does not seek simply to understand how sedimentary strata were formed; or how faults were produced; or how moraines originated; or how the beds of Alpine lakes were scooped out. But taking into account all agencies co-operating in endless and ever-varying combinations, he aims to interpret the entire structure of the Earth's crust. If he studies separately the actions of rain, rivers, glaciers, icebergs, tides, waves, volcanoes, earthquakes, etc.; he does so that he may be better able to comprehend their joint actions as factors in geological phenomena: the object of his science being to generalize these phenomena in all their involved connections, as parts of one whole. In like manner Biology is the elaboration of a complete theory of Life, in each and all of its involved manifestations. If different aspects of its phenomena are investigated apart—if one observer busies himself in classing organisms, another in dissecting them, another in ascertaining their chemical compositions, another in studying functions, another in tracing laws of modification; they are all, consciously or unconsciously, helping to work out a solution of vital phenomena in their entirety, both as displayed by individual organisms and by organisms at large. Thus, in these Concrete Sciences, the object is the converse of that which the Abstract-Concrete Sciences propose to themselves. In the one case we have *analytical interpretation;* while in the other case we have *synthetical interpretation.* Instead of synthesis

being used merely to verify analysis; analysis is here used only to aid synthesis. Not to formulate the factors of phenomena is now the object; but to formulate the phenomena resulting from these factors, under the various conditions which the Universe presents.

This third class of Sciences, like the other classes, is divisible into the universal and the non-universal. As there are truths which hold of all phenomena in their elements; so there are truths which hold of all phenomena in their totalities. As force has certain ultimate laws common to its separate modes of manifestation, so in those combinations of its modes which constitute actual phenomena, we find certain ultimate laws that are conformed to in every case. These are the laws of the re-distribution of force. Since we can become conscious of a phenomenon only by some change wrought in us, every phenomenon necessarily implies re-distribution of force—change in the arrangements of matter and motion. Alike in molecular movements and the movements of masses, one great uniformity may be traced. A decreasing quantity of motion, sensible or insensible, always has for its concomitant an increasing aggregation of matter; and, conversely, an increasing quantity of motion, sensible or insensible, has for its concomitant a decreasing aggregation of matter. Give to the molecules of any mass, more of that insensible motion which we call heat, and the parts of the mass become somewhat less closely aggregated. Add a further quantity of insensible motion,

and the mass so far disintegrates as to become liquid. Add still more insensible motion, and the mass disintegrates so completely as to become gas; which occupies a greater space with every extra quantity of insensible motion given to it. On the other hand, every loss of insensible motion by a mass, gaseous, liquid, or solid, is accompanied by a progressing integration of the mass. Similarly with sensible motions, be the bodies moved large or small. Augment the velocities of the planets, and their orbits will enlarge—the Solar System would occupy a wider space. Diminish their velocities, and their orbits will lessen—the Solar System will contract, or become more integrated. And in like manner we see that every sensible motion on the Earth's surface involves a partial disintegration of the moving body from the Earth; while the loss of its motion is accompanied by the body's re-integration with the Earth. In all phenomena we have either an integration of matter and concomitant dissipation of motion; or an absorption of motion and concomitant disintegration of matter. And where, as in living bodies, these processes are going on simultaneously, there is an integration of matter proportioned to the dissipation of motion, and an absorption of motion proportioned to the disintegration of matter. Such, then, are the universal laws of that re-distribution of matter and motion everywhere going on—a re-distribution which results in Evolution so long as

the aggregation of matter and dispersion of motion predominate; but which results in Dissolution where there is a predominant aggregation of motion and dispersion of matter. Hence we have a division of Concrete Science which bears towards the other Concrete Sciences, a relation like that which Universal Law of Relation bears to Mathematics, and like that which Universal Mechanics (composition and resolution of forces) bears to Physics. We have a division of Concrete Science which generalizes those concomitants of this re-distribution that hold good among all orders of concrete objects—a division which explains why, along with a predominating integration of matter and dissipation of motion, there goes a change from an indefinite, incoherent homogeneity, to a definite, coherent heterogeneity; and why a reverse re-distribution of matter and motion, is accompanied by a reverse structural change. Passing from this universal Concrete Science, to the non-universal Concrete Sciences; we find that these are primarily divisible into the science which deals with the re-distributions of matter and motion among the masses in space, consequent on their mutual actions as wholes; and the science which deals with the re-distributions of matter and motion consequent on the mutual actions of the molecules in each mass. And of these equally general Sciences, this last is re-divisible into the Science which is limited to the concomitants of re-distribution among the molecules of each mass when regarded as inde-

pendent, and the Science which takes into account the molecular motion received by radiation from other masses. But these sub-divisions, and their sub-sub-divisions, will be best seen in the annexed Table III.

That these great groups of Sciences and their respective sub-groups, fulfil the definition of a true classification given at the outset, is, I think, tolerably manifest. The subjects of inquiry included in each primary division, have essential attributes in common with one another, which they have not in common with any of the subjects contained in the other primary divisions; and they have, by consequence, a greater number of common attributes in which they severally agree with the colligated subjects, and disagree with the subjects otherwise colligated. Between Sciences which deal with relations apart from realities, and Sciences which deal with realities, the distinction is the widest possible; since Being, in some or all of its attributes, is common to all Sciences of the second class, and excluded from all Sciences of the first class. The distinction between the empty forms of things and the things themselves, is a distinction which cannot be exceeded in degree. And when we divide the Sciences which treat of realities, into those which deal with their separate components and those which deal with their components as united, we make a profounder distinction than can exist between the Sciences which deal with one or other order

TABLE III.

CONCRETE SCIENCE.

- Universal laws of the continuous re-distribution of Matter and Motion; which results in Evolution where there is a predominant integration of Matter and dissipation of Motion, and which results in Dissolution where there is a predominant absorption of Motion and disintegration of Matter.
- Laws of the redistributions of Matter and Motion actually going on
  - among the celestial bodies in their relations to one another as masses: comprehending (ASTRONOMY)
    - the dynamics of our stellar universe. (*Sidereal Astronomy.*)
    - the dynamics of our solar system. (*Planetary Astronomy.*)
  - among the molecules of any celestial mass; as caused by
    - the actions of these molecules on one another (ASTROGENY)
      - resulting in the formation of compound molecules. (*Solar Mineralogy.*)
      - resulting in molecular motions and genesis of radiant forces.*
      - resulting in movements of gases and liquids. (*Solar Meteorology.*†)
    - the actions of these molecules on one another, joined with the actions on them of forces radiated by the molecules of other masses: (GEOGENY)
      - as exhibited in the planets generally.
      - as exhibited in the Earth
        - causing composition and decomposition of inorganic matters. (*Mineralogy.*)
        - causing re-distributions of gases and liquids. (*Meteorology.*)
        - causing re-distributions of solids. (*Geology.*)
        - causing organic phenomena; which are (*Biology*)
          - those of structure (*Morphology*)
            - general.
            - special.
          - those of function
            - in their internal relations (*Physiology*)
              - general.
              - special.
            - in their external relations (*Psychology*)
              - general
              - special
                - separate.
                - combined. (*Sociology.*‡)

* This must not be supposed to mean chemically-produced forces. The molecular motion here referred to as dissipated in radiations, is the equivalent of that sensible motion lost during the integration of the mass of molecules, consequent on their mutual gravitation.

† Embracing the interpretation of such phenomena as the solar spots, the faculæ and the coronal flames.

‡ Want of space prevents anything beyond the briefest indication of these subdivisions.

of the components, or than can exist between the Sciences which deal with one or other order of the things composed. The three groups of Sciences may be briefly defined as—laws of the *forms;* laws of the *factors;* laws of the *products.* And when thus defined, it becomes manifest that the groups are so radically unlike in their natures, that there can be no transitions between them; and that any Science belonging to one of the groups must be quite incongruous with the Sciences belonging to either of the other groups, if transferred. How fundamental are the differences between them, will be further seen on considering their functions. The first, or abstract group, is *instrumental* with respect to both the others; and the second, or abstract-concrete group, is *instrumental* with respect to the third or concrete group. An endeavour to invert these functions will at once show how essential is the difference of character. The second and third groups supply subject-matter to the first, and the third supplies subject-matter to the second; but none of the truths which constitute the third group are of any use as solvents of the problems presented by the second group; and none of the truths which the second group formulates can act as solvents of problems contained in the first group. Concerning the subdivisions of these great groups, little remains to be added. That each of the groups, being co-extensive with all phenomena, contains truths that are universal

and others that are not universal, and that these must be classed apart, is obvious. And that the sub-divisions of the non-universal truths, are to be made in something like the manner shown in the tables, is proved by the fact that when the descriptive words are read from the root to the extremity of any branch, they form a definition of the Science constituting that branch. That the minor divisions might be otherwise arranged, and that better definitions of them might be given, is highly probable. They are here set down merely for the purpose of showing how this method of classification works out.

I will only further remark, that the relations of the Sciences as thus represented, are still but imperfectly represented: their relations cannot be truly shown on a plane, but only in space of three dimensions. The three groups cannot rightly be put in linear order as they have here been. Since the first stands related to the third, not only indirectly through the second, but also directly—it is directly instrumental with respect to the third, and the third supplies it directly with subject-matter. Their relations can thus only be truly shown by a divergence from a common root on different sides, in such a way that each stands in juxta-position to the other two. And only by the like mode of arrangement, can the relations among the sub-divisions of each group be correctly represented.

POSTSCRIPT,

REPLYING TO CRITICISMS.

Among objections made to any doctrine, those which come from avowed supporters of an adverse doctrine must be considered, other things equal, as of less weight than those which come from men uncommitted to an adverse doctrine, or but partially committed to it. The element of prepossession, distinctly present in the one case and in the other case mainly or quite absent, is a well-recognized cause of difference in the values of the judgments: supposing the judgments to be otherwise fairly comparable. Hence, when it is needful to bring the replies within a restricted space, a fit course is that of dealing rather with independent criticisms than with criticisms which are really indirect arguments for an opposite view, previously espoused.

For this reason I propose here to confine myself substantially, though not absolutely, to the demurrers entered against the foregoing classification by Prof. Bain, in his recent work on Logic. Before dealing with the more important of these, let me clear the ground by disposing of the less important.

Incidentally, while commenting on the view I take respecting the position of Logic, Prof. Bain points out that this, which is the most abstract of the sciences, owes much to Psychology, which I place among the Concrete Sciences; and he alleges an incongruity between this fact and my statement that the Concrete Sciences are not instrumental

in disclosing the truths of the Abstract Sciences. Subsequently he re-raises this apparent anomaly when saying—

"Nor is it possible to justify the placing of Psychology wholly among Concrete Sciences. It is a highly analytic science, as Mr. Spencer thoroughly knows."

For a full reply, given by implication, I must refer Prof. Bain to § 56 of *The Principles of Psychology*, where I have contended that "while, under its objective aspect, Psychology is to be classed as one of the Concrete Sciences which successively decrease in scope as they increase in speciality; under its subjective aspect, Psychology is a totally unique science, independent of, and antithetically opposed to, all other sciences whatever." A pure idealist will not, I suppose, recognize this distinction; but to every one else it must, I should think, be obvious that the science of subjective existences is the correlative of all the sciences of objective existences; and is as absolutely marked off from them as subject is from object. Objective Psychology, which I class among the Concrete Sciences, is purely synthetic, so long as it is limited, like the other sciences, to objective data; though great aid in the interpretation of these data is derived from the observed correspondence between the phenomena of Objective Psychology as presented in other beings and the phenomena of Subjective Psychology as presented in one's own consciousness. Now it is Subjective Psychology only which is analytic, and which affords aid in the development of Logic. This being explained, the apparent incongruity disappears.

A difficulty raised respecting the manner in which I have expressed the nature of Mathematics, may next be dealt with. Prof. Bain writes:—

"In the first place, objection may be taken to his language, in discussing the extreme Abstract Sciences, when he speaks of the *empty forms* therein considered. To call Space and Time empty

forms, must mean that they can be thought of without any concrete embodiment whatsoever; that one can think of Time, as a pure abstraction, without having in one's mind any concrete succession. Now, this doctrine is in the last degree questionable."

I quite agree with Prof. Bain that "this doctrine is in the last degree questionable;" but I do not admit that this doctrine is implied by the definition of Abstract Science which I have given. I speak of Space and Time as they are dealt with by mathematicians, and as it is alone possible for pure Mathematics to deal with them. While Mathematics habitually uses in its points, lines, and surfaces, certain existences, it habitually deals with these as representing points, lines, and surfaces that are ideal; and *its conclusions are true only on condition that it does this.* Points having dimensions, lines having breadths, planes having thicknesses, are negatived by its definitions. Using, though it does, material representatives of extension, linear, superficial, or solid, Geometry deliberately ignores their materiality; and attends only to the truths of relation they present. Holding with Prof. Bain, as I do, that our consciousness of Space is disclosed by our experiences of Matter—arguing, as I have done in *The Principles of Psychology*, that it is a consolidated aggregate of all relations of co-existence that have been severally presented by Matter; I nevertheless contend that it is possible to dissociate these relations from Matter to the extent required for formulating them as abstract truths. I contend, too, that this separation is of the kind habitually made in other cases; as, for instance, when the general laws of motion are formulated (as M. Comte's system, among others, formulates them) in such way as to ignore all properties of the bodies dealt with save their powers of taking up, and retaining, and giving out, quantities of motion; though these powers are inconceivable apart from the attribute of extension, which is intentionally disregarded.

Taking other of Prof. Bain's objections, not in the order in which they stand but in the order in which they may be most conveniently dealt with, I quote as follows:—

"The law of the radiation of light (the inverse square of the distance) is said by Mr. Spencer to be Abstract-Concrete, while the disturbing changes in the medium are not to be mentioned except in a Concrete Science of Optics. We need not remark that such a separate handling is unknown to science."

It is perfectly true that "such a separate handling is unknown to science." But, unfortunately for the objection, it is also perfectly true that no such separate handling is proposed by me, or is implied by my classification. How Prof. Bain can have so missed the meaning of the word "concrete," as I have used it, I do not understand. After pointing out that "no one ever drew the line," between the Abstract-Concrete and the Concrete Sciences, "as I have done it," he alleges an anomaly which exists only supposing that I have drawn it where it is ordinarily drawn. He appears inadvertently to have carried with him M. Comte's conception of Optics as a Concrete Science, and, importing it into my classification, debits me with the incongruity. If he will re-read the definition of the Abstract-Concrete Sciences, or study their sub-divisions as shown in Table II., he will, I think, see that the most special laws of the redistribution of light, equally with its most general laws, are included. And if he will pass to the definition and the tabulation of the Concrete Sciences, he will, I think, see no less clearly that Optics cannot be included among them.

Prof. Bain considers that I am not justified in classing Chemistry as an Abstract-Concrete Science, and excluding from it all consideration of the crude forms of the various substances dealt with; and he enforces his dissent by saying that chemists habitually describe the ores and impure mixtures in which the elements, etc., are naturally found. Undoubtedly chemists do this. But do they therefore intend

to include an account of the ores of a substance, *as a part of the science* which formulates its molecular constitution and the constitutions of all the definite compounds it enters into? I shall be very much surprised if I find that they do. Chemists habitually prefix to their works a division treating of Molecular Physics; but they do not therefore claim Molecular Physics as a part of Chemistry. If they similarly prefix to the chemistry of each substance an outline of its mineralogy, I do not think they therefore mean to assert that the last belongs to the first. Chemistry proper, embraces nothing beyond an account of the constitutions and modes of action and combining proportions of substances that are taken as absolutely pure; and its truths no more recognize impure substances than the truths of Geometry recognize crooked lines.

Immediately after, in criticizing the fundamental distinction I have made between Chemistry and Biology, as Abstract-Concrete and Concrete respectively, Prof. Bain says:—

> "But the objects of Chemistry and the objects of Biology are equally concrete, so far as they go; the simple bodies of chemistry, and their several compounds, are viewed by the Chemist as concrete wholes, and are described by him, not with reference to one factor, but to all their factors."

Issue is here raised in a form convenient for elucidation of the general question. It is true that, *for purposes of identification*, a chemist gives an account of all the sensible characters of a substance. He sets down its crystalline form, its specific gravity, its power of refracting light, its behaviour as magnetic or diamagnetic. But does he thereby include these phenomena as part of the Science of Chemistry? It seems to me that the relation between the weight of any portion of matter and its bulk, which is ascertained on measuring its specific gravity, is a physical and not a chemical fact. I think, too, that the physicist

will claim, as part of his science, all investigations touching the refraction of light: be the substance producing this refraction what it may. And the circumstance that the chemist may test the magnetic or diamagnetic property of a body, as a means of ascertaining what it is, or as a means of helping other chemists to determine whether they have got before them the same body, will neither be held by the chemist, nor allowed by the physicist, to imply a transfer of magnetic phenomena from the domain of the one to that of the other. In brief, though the chemist, in his account of an element or a compound, may refer to certain physical traits associated with its molecular constitution and affinities, he does not by so doing change these into chemical traits. Whatever chemists may put into their books, Chemistry, considered as a science, includes only the phenomena of molecular structures and changes—of compositions and decompositions.* I contend, then, that Chemistry does *not* give an account of anything as a concrete whole, in the same way that Biology gives an account of an organism as a concrete whole. This will become even more manifest on observing the character of the biological account. All the attributes of an organism are comprehended, from the most general to the most special—from its conspicuous structural traits to its hidden and faint ones; from its outer actions that thrust themselves on the attention, to the minutest sub-divisions of its multitudinous

* Perhaps some will say that such incidental phenomena as those of the heat and light evolved during chemical changes, are to be included among chemical phenomena. I think, however, the physicist will hold that all phenomena of re-distributed molecular motion, no matter how arising, come within the range of Physics. But whatever difficulty there may be in drawing the line between Physics and Chemistry (and, as I have incidentally pointed out in *The Principles of Psychology*, § 55, the two are closely linked by the phenomena of allotropy and isomerism), applies equally to the Comtean classification, or to any other. And I may further point out that no obstacle hence arises to the classification I am defending. Physics and Chemistry being both grouped by me as Abstract-Concrete Sciences, no difficulty in satisfactorily dividing them in the least affects the satisfactoriness of the division of the great group to which they both belong, from the other two great groups.

internal functions; from its character as a germ, through the many changes of size, form, organization, and habit, it goes through until death; from the physical characters of it as a whole, to the physical characters of its microscopic cells, and vessels, and fibres; from the chemical characters of its substance in general to the chemical characters of each tissue and each secretion—all these, with many others. And not only so, but there is comprehended as the ideal goal of the science, the *consensus* of all these phenomena in their co-existences and successions, as constituting a coherent individualized group definitely combined in space and in time. It is this recognition of *individuality* in its subject-matter, that gives its concreteness to Biology, as to every other Concrete Science. As Astronomy deals with bodies that have their several proper names, or (as with the smaller stars) are registered by their positions, and considers each of them as a distinct individual—as Geology, while dimly perceiving in the Moon and nearest planets other groups of geological phenomena (which it would deal with as independent wholes, did not distance forbid), occupies itself with that individualized group presented by the Earth; so Biology treats either of an individual distinguished from all others, or of parts or products belonging to such an individual, or of structural or functional traits common to many such individuals that have been observed, and supposed to be common to others that are like them in most or all of their attributes. Every biological truth connotes a specifically individualized object, or a number of specifically individualized objects of the same kind, or numbers of different kinds that are severally specific. See, then, the contrast. The truths of the Abstract-Concrete Sciences do not imply specific individuality. Neither Molar Physics, nor Molecular Physics, nor Chemistry, concerns itself with this. The laws of motion are expressed without any reference whatever to the sizes or shapes of the moving

masses; which may be taken indifferently to be suns or atoms. The relations between contraction and the escape of molecular motion, and between expansion and the absorption of molecular motion, are expressed in their general forms without reference to the kind of matter; and, if the degree of either that occurs in a particular kind of matter is formulated, no note is taken of the quantity of that matter, much less of its individuality. Similarly with Chemistry. When it inquires into the atomic weight, the molecular structure, the atomicity, the combining proportions, etc., of a substance, it is indifferent whether a grain or a ton be thought of—the conception of amount is absolutely irrelevant. And so with more special attributes. Sulphur, considered chemically, is not sulphur under its crystalline form, or under its allotropic viscid form, or as a liquid, or as a gas; but it is sulphur considered apart from those attributes of quantity, and shape, and state, that give individuality.

Prof. Bain objects to the division I have drawn between the Concrete Science of Astronomy and that Abstract-Concrete Science which deals with the mutually-modified motions of hypothetical masses in space, as "not a little arbitrary." He says:—

"We can suppose a science to confine itself *solely* to the 'factors,' or the separated elements, and never, on any occasion, to combine two into a composite third. This position is intelligible, and possibly defensible. For example, in Astronomy, the Law of Persistence of Motion in a straight line might be discussed in pure ideal separation; and so, the Law of Gravity might be discussed in equally pure separation—both under the Abstract-Concrete department of Mechanics. It might then be reserved to a *concrete* department to unite these in the explanation of a projectile or of a planet. Such, however, is not Mr. Spencer's boundary line. He allows Theoretical Mechanics to make this particular combination, and to arrive at the laws of planetary movement, *in the case of a single planet.* What he does not allow is, to proceed to the case of two planets, mutually disturbing one another, or a planet and a satellite, commonly called the 'problem of the Three Bodies.'"

If I held what Prof. Bain supposes me to hold, my position would be an absurd one; but he misapprehends me. The misapprehension results in part from his having here, as before, used the word "concrete" with the Comtean meaning, as though it were my meaning; and in part from the inadequacy of my explanation. I did not in the least mean to imply that the Abstract-Concrete Science of Mechanics, when dealing with the motions of bodies in space, is limited to the interpretation of planetary movement such as it would be did only a single planet exist. It never occurred to me that my words (see p. 19) might be so construed. Abstract-Concrete problems admit, in fact, of being complicated indefinitely, without going in the least beyond the definition. I do not draw the line, as Prof. Bain alleges, between the combination of two factors and the combination of three, or between the combination of any number and any greater number. I draw the line between the science which deals with the theory of the factors, taken singly and in combinations of two, three, four, or more, and the science which, *giving to these factors the values derived from observations of actual objects, uses the theory to explain actual phenomena.*

It is true that, in these departments of science, no radical distinction is consistently recognized between theory and the applications of theory. As Prof. Bain says:—

"Newton, in the First Book of the Principia, took up the problem of the Three Bodies, as applied to the Moon, and worked it to exhaustion. So writers on Theoretical Mechanics continue to include the Three Bodies, Precession, and the Tides."

But, supreme though the authority of Newton may be as a mathematician and astronomer, and weighty as are the names of Laplace and Herschel, who in their works have similarly mingled theorems and the explanations yielded by them, it does not seem to me that these facts go for much; unless it can be shown that these writers intended thus to enunciate the views at which they had arrived respecting the classifi-

cation of the sciences. Such a union as that presented in their works, adopted merely for the sake of convenience, is, in fact, the indication of incomplete development; and has been paralleled in simpler sciences which have afterwards outgrown it. Two conclusive illustrations are at hand. The name Geometry, utterly inapplicable by its meaning to the science as it now exists, was applicable in that first stage when its few truths were taught in preparation for land-measuring and the setting-out of buildings; but, at a comparatively early date, these comparatively simple truths became separated from their applications, and were embodied by the Greek geometers into systems of theory.* A like purification is now taking place in another division of the science. In the *Géométrie Descriptive* of Monge, theorems were mixed with their applications to projection and plan-drawing. But, since his time, the science and the art have been segregating; and Descriptive Geometry, or, as it may be better termed, the Geometry of Position, is now recognized by mathematicians as a far-reaching system of truths, parts of which are already embodied in books that make no reference to derived methods available by the architect or the engineer. To meet a counter-illustration that will be cited, I may remark that though, in works on Algebra intended for beginners, the theories of quantitative relations, as treated algebraically, are accompanied by groups of problems to be solved, the subject-matters of these problems are not thereby made parts of the Science of Algebra. To say that they are, is to say that Algebra includes the conceptions of distances and relative speeds and times, or of weights and bulks and specific gravities, or of areas ploughed and days and wages; since these, and endless others, may be the terms of

* It may be said that the mingling of problems and theorems in Euclid is not quite consistent with this statement; and it is true that we have, in this mingling, a trace of the earlier form of the science. But it is to be remarked that these problems are all purely abstract, and, further, that each of them admits of being expressed as a theorem.

its equations. And just in the same way that these concrete problems, solved by its aid, cannot by any possibility be incorporated with the Abstract Science of Algebra; so I contend that the concrete problems of Astronomy, cannot by any possibility be incorporated with that division of Abstract-Concrete Science which develops the theory of the inter-actions of free bodies that attract one another.

On this point I find myself at issue, not only with Prof. Bain, but also with Mr. Mill, who contends that :—

> "There *is* an abstract science of astronomy, namely, the theory of gravitation, which would equally agree with and explain the facts of a totally different solar system from the one of which our earth forms a part. The actual facts of our own system, the dimensions, distances, velocities, temperatures, physical constitution, etc., of the sun, earth, and planets, are properly the subject of a concrete science, similar to natural history; but the concrete is more inseparably united to the abstract science than in any other case, since the few celestial facts really accessible to us are nearly all required for discovering and proving the law of gravitation as an universal property of bodies, and have therefore an indispensable place in the abstract science as its fundamental data."—*Auguste Comte and Positivism*, p. 43.

In this explanation, Mr. Mill recognizes the fundamental distinction between the Concrete Science of Astronomy, dealing with the bodies actually distributed in space, and a science dealing with hypothetical bodies hypothetically distributed in space. Nevertheless, he regards these sciences as not separable; because the second derives from the first the data whence the law of inter-action is derived. But the truth of this premiss, and the legitimacy of this inference, may alike be questioned. The discovery of the law of inter-action was not due primarily, but only secondarily, to observation of the heavenly bodies. The conception of an inter-acting force that varies inversely as the square of the distance, is an *à priori* conception rationally deducible from mechanical and geometrical considerations. Though unlike in derivation to the many empirical hypotheses of Kepler

respecting planetary orbits and planetary motions, yet it was like the successful among these in its relation to astronomical phenomena: it was one of many possible hypotheses, which admitted of having their consequences worked out and tested; and one which, on having its implications compared with the results of observation, was found to explain them. In short, the theory of gravitation grew out of experiences of terrestrial phenomena; but the verification of it was reached through experiences of celestial phenomena. Passing now from premiss to inference, I do not see that, even were the alleged parentage substantiated, it would necessitate the supposed inseparability; any more than the descent of Geometry from land-measuring necessitates a persistent union of the two. In the case of Algebra, as above indicated, the disclosed laws of quantitative relations hold throughout multitudinous orders of phenomena that are extremely heterogeneous; and this makes conspicuous the distinction between the theory and its applications. Here the laws of quantitative relations among masses, distances, velocities, and momenta, being applied mainly (though not exclusively) to the concrete cases presented by Astronomy, the distinction between the theory and its applications is less conspicuous. But, intrinsically, it is as great in the one case as in the other.

How great it is, we shall see on taking an analogy. This is a living man, of whom we may know little more than that he is a visible, tangible person; or of whom we may know enough to form a voluminous biography. Again, this book tells of a fictitious hero, who, like the heroes of old romance, may be an impersonated virtue or vice, or, like a modern hero, one of mixed nature, whose various motives and consequent actions are elaborated into a semblance of reality. But no accuracy and completeness of the picture makes this fictitious personage an actual personage, or brings him any nearer to one. Nor does any meagreness in our knowledge

of a real man reduce him any nearer to the imaginary being of a novel. To the last, the division between fiction and biography remains an impassable gulf. So, too, remains the division between the Science dealing with the inter-actions of hypothetical bodies in space, and the Science dealing with the inter-actions of existing bodies in space. We may elaborate the first to any degree whatever by the introduction of three, four, or any greater number of factors under any number of assumed conditions, until we symbolize a solar system; but to the last an account of our symbolic solar system is as far from an account of the actual solar system as fiction is from biography.

Even more obvious, if it be possible, does the radical character of this distinction become, on observing that from the simplest proposition of General Mechanics we may pass to the most complex proposition of Celestial Mechanics, without a break. We take a body moving at a uniform velocity, and commence with the proposition that it will continue so to move for ever. Next, we state the law of its accelerated motion in the same line, when subject to a uniform force. We further complicate the proposition by supposing the force to increase in consequence of approach towards an attracting body; and we may formulate a series of laws of acceleration, resulting from so many assumed laws of increasing attraction (of which the law of gravitation is one). Another factor may now be added by supposing the body to have motion in a direction other than that of the attracting body; and we may determine, according to the ratios of the supposed forces, whether its course will be hyperbolic, parabolic, elliptical, or circular—we may begin with this hypothetical additional force as infinitesimal, and formulate the varying results as it is little by little increased. The problem is complicated a degree more by taking into account the effects of a third force, acting in some other direction; and beginning with an infinitesimal amount of this force we may

reach any amount. Similarly, by introducing factor after factor, each at first insensible in proportion to the rest, we arrive, through an infinity of gradations, at a combination of any complexity.

Thus, then, the Science which deals with the inter-action of hypothetical bodies in space, is *absolutely continuous* with General Mechanics. We have already seen that it is *absolutely discontinuous* with that account of the heavenly bodies which has been called Astronomy from the beginning. When these facts are recognized, it seems to me that there cannot remain a doubt respecting its true place in a classification of the Sciences.

Passing over minor criticisms, either as met by implication or as demanding space that cannot be here afforded, let me say something by way of enforcing the general argument. I will re-state the case in two ways: the first of them adapted only to those who accept the general doctrine of Evolution.

We set out with concentrating nebulous matter. Tracing the re-distributions of this as the rotating contracting spheroid leaves behind successive annuli, and as these severally breaking up eventually form secondary rotating spheroids, we come at length to planets in their early stages. Thus far we consider the phenomena dealt with purely astronomical; and so long as our Earth, regarded as one of these spheroids, was made up of gaseous and molten matters only, it presented no definite data for any more complex Concrete Science. In the lapse of cosmical time a solid film forms, which, in the course of millions of years, thickens, and, in the course of further millions of years, becomes cool enough to permit the precipitation, first of various other gaseous compounds, and finally of water. Presently, the varying exposure of different parts of the spheroid to the Sun's rays, begins to produce appreciable

effects; until at length there have arisen meteorological actions, and consequent geological actions, such as those we now know: determined partly by the Sun's heat, partly by the still-retained internal heat of the Earth, and partly by the action of the Moon on the ocean? How have we reached these geological phenomena? When did the astronomical changes end and the geological begin? It needs but to ask this question to see that there is no real division between the two. Putting pre-conceptions aside, we find nothing more than a group of phenomena continually complicating under the influence of the same original factors; and we see that our conventional division is defensible only on grounds of convenience. Let us advance a stage. As the Earth's surface continues to cool, passing through all degrees of temperature by infinitesimal gradations, the formation of more and more complex inorganic compounds becomes possible; later its surface sinks to that heat at which the less complex compounds of the kinds called organic can exist; and finally the formation of the more complex organic compounds becomes possible. Chemists now show us that these compounds may be built up synthetically in the laboratory—each stage in ascending complexity making possible the next higher stage. Hence it is inferable that, in the myriads of laboratories, endlessly diversified in their materials and conditions, which the Earth's surface furnished during the myriads of years occupied in passing through these stages of temperature, such successive syntheses were effected; and that the highly complex unstable substance out of which all organisms are composed, was eventually formed in microscopic portions: from which, by continuous integrations and differentiations, the evolution of all organisms has proceeded. Where then shall we draw the line between Geology and Biology? The synthesis of this most complex compound, is but a continuation of the syntheses by which all simpler compounds were formed.

The same primary factors have been co-operating with those secondary factors, meteorologic and geologic, previously derived from them. Nowhere do we find a break in the ever-complicating series; for there is a manifest connexion between those movements which various complex compounds undergo during their isomeric transformations, and those changes of form undergone by the protoplasm which we distinguish as living. Strongly contrasted as they eventually become, biological phenomena are at their root inseparable from geological phenomena—inseparable from the aggregate of transformations continually wrought in the matters forming the Earth's surface by the physical forces to which they are exposed. Further stages I need not particularize. The gradual development out of the biological group of phenomena, of the more specialized group we class as psychological, needs no illustration. And when we come to the highest psychological phenomena, it is clear that since aggregations of human beings may be traced upwards from single wandering families to tribes and nations of all sizes and complexities, we pass insensibly from the phenomena of individual human action to those of corporate human action. To resume, then, is it not manifest that in the group of sciences—Astronomy, Geology, Biology, Psychology, Sociology, we have a natural group that admits neither of disruption nor change of order? Here there is both a genetic dependence, and a dependence of interpretations. The phenomena have arisen in this succession in cosmical time; and complete scientific interpretation of each group depends on scientific interpretation of the preceding groups. No other science can be thrust in anywhere without destroying the continuity. To insert Physics between Astronomy and Geology, would be to make a break in the history of a continuous series of changes; and a like break would be produced by inserting Chemistry between Geology and Biology. It is true that Physics and Chemistry are

needful as interpreters of these successive assemblages of facts; but it does not therefore follow that they are themselves to be placed among these assemblages.

Concrete Science, made up of these five concrete sub-sciences, being thus coherent within itself, and separated from all other science, there comes the question—Is all other science similarly coherent within itself? or is it traversed by some second division that is equally decided? It is thus traversed. A statical or dynamical theorem, however simple, has always for its subject-matter something that is conceived as extended, and as displaying force or forces—as being a seat of resistance, or of tension, or of both, and as capable of possessing more or less of *vis viva.* If we examine the simplest proposition of Statics, we see that the conception of Force must be joined with the conception of Space, before the proposition can be framed in thought; and if we similarly examine the simplest proposition in Dynamics, we see that Force, Space, and Time, are its essential elements. The amounts of the terms are indifferent; and, by reduction of its terms beyond the limits of perception, they are applied to molecules: Molar Mechanics and Molecular Mechanics are continuous. From questions concerning the relative motions of two or more molecules, Molecular Mechanics passes to changes of aggregation among many molecules, to changes in the amounts and kinds of the motions possessed by them as members of an aggregate, and to changes of the motions transferred through aggregates of them (as those constituting light). Daily extending its range of interpretations, it is coming to deal even with the components of each compound molecule on the same principles. And the unions and disunions of such more or less compound molecules, which constitute the phenomena of Chemistry, are also being conceived as resultant phenomena of essentially kindred natures—the affinities of molecules for one another, and their reactions in relation to light, heat, and other modes of force,

being regarded as consequent on the combinations of the various mechanically-determined motions of their various components. Without at all out-running, however, this progress in the mechanical interpretation of molecular phenomena, it suffices to point out that the indispensable elements in any chemical conception are units occupying places in space, and exerting forces on one another. This, then, is the common character of all these sciences which we at present group under the names of Mechanics, Physics, Chemistry. Leaving undiscussed the question whether it is possible to conceive of force apart from extended somethings exerting it, we may assert, as beyond dispute, that if the conception of force be expelled, no science of Mechanics, Physics, or Chemistry remains. Made coherent, as these sciences are, by this bond of union, it is impossible to thrust among them any other science without breaking their continuity. We cannot place Logic between Molar Mechanics and Molecular Mechanics. We cannot place Mathematics between the group of propositions concerning the behaviour of homogeneous molecules to one another, and the group of propositions concerning the behaviour of heterogeneous molecules to one another (which we call Chemistry). Clearly these two sciences lie outside the coherent whole we have contemplated: separated from it in some radical way.

By what are they radically separated? By the absence of the conception of force. However true it may be that so long as Logic and Mathematics have any terms at all, these must be capable of affecting consciousness, and, by implication, of exerting force; yet it is the distinctive trait of these sciences that not only do their propositions make no reference to such force, but, as far as possible, they deliberately ignore it. Instead of being, as in all the other sciences, an element that is not only recognized but vital; in Mathematics and Logic, force is an element that is not only not vital, but is studiously not recognized. The terms in

which Logic expresses its propositions, are symbols that do not profess to represent things, properties, or powers, of one kind more than another; and may equally well stand for the attributes belonging to members of some connected series of ideal curves which have never been drawn, as for so many real objects. And the theorems of Geometry, so far from contemplating perceptible lines and surfaces as elements in the truths enunciated, consider these truths as becoming absolute only when such lines and surfaces become ideal—only when the conception of something exercising force is extruded.

Let me now make a second re-statement, not implying acceptance of the doctrine of Evolution, but exhibiting with a clearness almost if not quite as great, these fundamental distinctions.

The concrete sciences, taken together or separately, contemplate as their subject-matters, *aggregates*—either the entire aggregate of sensible existences, or some secondary aggregate separable from this entire aggregate, or some tertiary aggregate separable from this, and so on. Sidereal Astronomy occupies itself with the totality of visible masses distributed through space; which it deals with as made up of identifiable individuals occupying specified places, and severally standing towards one another, towards sub-groups, and towards the entire group, in defined ways. Planetary Astronomy, cutting out of this all-including aggregate that relatively minute part constituting the Solar System, deals with this as a whole—observes, measures, and calculates the sizes, shapes, distances, motions, of its primary, secondary, and tertiary members; and, taking for its larger inquiries the mutual actions of all these members as parts of a co-ordinated assemblage, takes for its smaller inquiries the actions of each member considered as an individual, having a set of intrinsic activities that are modified by a set of

extrinsic activities. Restricting itself to one of these aggregates, which admits of close examination, Geology (using this word in its comprehensive meaning) gives an account of terrestrial actions and terrestrial structures, past and present; and, taking for its narrower problems local formations and the agencies to which they are due, takes for its larger problems the serial transformations undergone by the entire Earth. The geologist being occupied with this cosmically small, but otherwise vast, aggregate, the biologist occupies himself with small aggregates formed out of parts of the Earth's superficial substance, and treats each of these as a coordinated whole in its structures and functions; or, when he treats of any particular organ, considers this as a whole made up of parts held in a sub-coordination that refers to the coordination of the entire organism. To the psychologist he leaves those specialized aggregates of functions which adjust the actions of organisms to the complex activities surrounding them: doing this, not simply because they are a stage higher in speciality, but because they are the counterparts of those aggregated states of consciousness dealt with by the science of Subjective Psychology, which stands entirely apart from all other sciences. Finally, the sociologist considers each tribe and nation as an aggregate presenting multitudinous phenomena, simultaneous and successive, that are held together as parts of one combination. Thus, in every case, a concrete science deals with a real aggregate (or a plurality of such aggregates); and it includes as its subject-matter whatever is to be known of this aggregate in respect of its size, shape, motions, density, texture, general arrangement of parts, minute structure, chemical composition, temperature, etc., together with all the multitudinous changes, material and dynamical, gone through by it from the time it begins to exist as an aggregate to the time it ceases to exist as an aggregate.

No abstract-concrete science makes the remotest attempt

to do anything of this sort. Taken together, the abstract-concrete sciences give an account of the various kinds of *properties* which aggregates display; and each abstract-concrete science concerns itself with a certain order of these properties. By this, the properties common to all aggregates are studied and formulated; by that, the properties of aggregates having special forms, special states of aggregation, etc.; and by others, the properties of particular components of aggregates when dissociated from other components. But by all these sciences the aggregate, considered as an individual object, is tacitly ignored; and a property, or a connected set of properties, exclusively occupies attention. It matters not to Mechanics whether the moving mass it considers is a planet or a molecule, a dead stick thrown into the river or the living dog that leaps after it: in any case the curve described by the moving mass conforms to the same laws. Similarly when the physicist takes for his subject the relation between the changing bulk of matter and the changing quantity of molecular motion it contains. Dealing with the subject generally, he leaves out of consideration the kind of matter; and dealing with the subject specially in relation to this or that kind of matter, he ignores the attributes of size and form: save in the still more special cases where the effect on form is considered, and even then size is ignored. So, too, is it with the chemist. A substance he is investigating, never thought of by him as distinguished in extension or amount, is not even required to be perceptible. A portion of carbon on which he is experimenting, may or may not have been visible under its forms of diamond or graphite or charcoal—this is indifferent. He traces it through various disguises and various combinations—now as united with oxygen to form an invisible gas; now as hidden with other elements in such more complex compounds as ether, and sugar, and oil. By sulphuric acid or other agent he precipitates it from these

as a coherent cinder, or as a diffused impalpable powder; and again, by applying heat, forces it to disclose itself as an element of animal tissue. Evidently, while thus ascertaining the affinities and atomic equivalence of carbon, the chemist has nothing to do with any aggregate. He deals with carbon in the abstract, as something considered apart from quantity, form, appearance, or temporary state of combination; and conceives it as the possessor of powers or properties, whence the special phenomena he describes result: the ascertaining of all these powers or properties being his sole aim.

Finally, the Abstract Sciences ignore alike aggregates and the powers which aggregates or their components possess; and occupy themselves with *relations*—either with the relations among aggregates, or among their parts, or the relations among aggregates and properties, or the relations among properties, or the relations among relations. The same logical formula applies equally well, whether its terms are men and their deaths, crystals and their planes of cleavage, or letters and their sounds. And how entirely Mathematics concerns itself with relations, we see on remembering that it has just the same expression for the characters of an infinitesimal triangle, as for those of the triangle which has Sirius for its apex and the diameter of the Earth's orbit for its base.

I cannot see how these definitions of these groups of sciences can be questioned. It is undeniable that every Concrete Science gives an account of an aggregate or of aggregates, inorganic, organic, or super-organic (a society); and that, not concerning itself with properties of this or that order, it concerns itself with the co-ordination of the assembled properties of all orders. It seems to me no less certain that an Abstract-Concrete Science gives an account of some order of properties, general or special; not caring about the other traits of an aggregate displaying them, and not

recognizing aggregates at all further than is implied by discussion of the particular order of properties. And I think it is equally clear that an Abstract Science, freeing its propositions, so far as the nature of thought permits, from aggregates and properties, occupies itself with the relations of co-existence and sequence, as disentangled from all particular forms of being and action. If then these three groups of sciences are, respectively, accounts of *aggregates*, accounts of *properties*, accounts of *relations*, it is manifest that the divisions between them are not simply perfectly clear, but that the chasms between them are absolute.

Here, perhaps more clearly than before, will be seen the untenability of the classification made by M. Comte. Already (p. 11), after setting forth in a general way these fundamental distinctions, I have pointed out the incongruities that arise when the sciences, conceived as Abstract, Abstract-Concrete, and Concrete, are arranged in the order proposed by him. Such incongruities become still more conspicuous if for these general names of the groups we substitute the definitions given above. The series will then stand thus:—

MATHEMATICS ....An account of *relations*
(including, under Mechanics, an account of *properties*).
ASTRONOMY ....An account of *aggregates*.
PHYSICS ....An account of *properties*.
CHEMISTRY ....An account of *properties*.
BIOLOGY ....An account of *aggregates*.
SOCIOLOGY ....An account of *aggregates*.

That those who espouse opposite views see clearly the defects in the propositions of their opponents and not those in their own, is a trite remark that holds in philosophical discussions as in all others: the parable of the mote and

the beam applies as well to men's appreciations of one another's opinions as to their appreciations of one another's natures. Possibly to my positivist friends I exemplify this truth,—just as they exemplify it to me. Those uncommitted to either view must decide where the mote exists and where the beam. Meanwhile it is clear that one or other of the two views is essentially erroneous; and that no qualifications can bring them into harmony. Either the sciences admit of no such grouping as that which I have described, or they admit of no such serial order as that given by M. Comte.

London,
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II.

REASONS FOR DISSENTING FROM THE PHILOSOPHY OF M. COMTE.

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PHILOSOPHY OF M. COMTE.

While the preceding pages were passing through the press, there appeared in the *Revue des Deux Mondes* for February 15th, an article on a late work of mine—*First Principles*. To M. Auguste Laugel, the writer of this article, I am much indebted for the careful exposition he has made of some of the leading views set forth in that work; and for the catholic and sympathetic spirit in which he has dealt with them. In one respect, however, M. Laugel conveys to his readers an erroneous impression—an impression doubtless derived from what appears to him adequate evidence, and doubtless expressed in perfect sincerity. M. Laugel describes me as being, in part, a follower of M. Comte. After describing the influence of M. Comte as traceable in the works of some other English writers, naming especially Mr. Mill and Mr. Buckle, he goes on to say that this influence, though not avowed, is easily recognizable in the work he is about to make known; and in several places throughout his review, there are remarks having the same implication. I greatly regret having to take exception to anything said by a critic so candid and so able. But the *Revue des Deux Mondes* circulates widely in England, as well as elsewhere; and finding that there exists in some minds, both here and in America, an impression similar to that entertained by M. Laugel—an impression likely to be confirmed by his statement—it appears to me needful to meet it.

Two causes of quite different kinds, have conspired to diffuse the erroneous belief that M. Comte is an accepted exponent of scientific opinion. His bitterest foes and his closest friends, have unconsciously joined in propagating it. On the one hand, M. Comte having designated by the term "Positive Philosophy" all that definitely-established knowledge which men of science have been gradually organizing into a coherent body of doctrine; and having habitually placed this in opposition to the incoherent body of doctrine defended by theologians; it has become the habit of the theological party to think of the antagonist scientific party, under the title of "positivists." And thus, from the habit of calling them "positivists," there has grown up the assumption that they call themselves "positivists," and that they are the disciples of M. Comte. On the other hand, those who have accepted M. Comte's system, and believe it to be the philosophy of the future, have naturally been prone to see everywhere the signs of its progress; and wherever they have found opinions in harmony with it, have ascribed these opinions to the influence of its originator. It is always the tendency of discipleship to magnify the effects of the master's teachings; and to credit the master with all the doctrines he teaches. In the minds of his followers, M. Comte's name is associated with scientific thinking, which, in many cases, they first understood from his exposition of it. Influenced as they inevitably are by this association of ideas, they are reminded of M. Comte wherever they meet with thinking which corresponds, in some marked way, to M. Comte's description of scientific thinking; and hence are apt to imagine him as introducing into other minds, the conceptions which he introduced into their minds. Such impressions are, however, in most cases quite unwarranted. That M. Comte has given a general exposition of the doctrine and method elaborated by Science, is true. But it is not true that the holders of this doctrine and followers of this method,

are disciples of M. Comte. Neither their modes of inquiry nor their views concerning human knowledge in its nature and limits, are appreciably different from what they were before. If they are "positivists," it is in the sense that all men of science have been more or less consistently "positivists;" and the applicability of M. Comte's title to them, no more makes them his disciples, than does its applicability to men of science who lived and died before M. Comte wrote, make these his disciples. M. Comte himself by no means claims that which some of his adherents are apt, by implication, to claim for him. He says:—"Il y a, sans doute, beaucoup d'analogie entre ma *philosophie positive* et ce que les savans anglais entendent, depuis Newton surtout, par *philosophie naturelle*;" (see *Avertissement)* and further on he indicates the "grand mouvement imprimé à l'esprit humain, il y a deux siècles, par l'action combinée des préceptes de Bacon, des conceptions de Descartes, et des découvertes de Galileé, comme le moment où l'esprit de la philosophie positive a commencé à se prononcer dans le monde." That is to say, the general mode of thought and way of interpreting phenomena, which M. Comte calls "Positive Philosophy," he recognizes as having been growing for two centuries; as having reached, when he wrote, a marked development; and as being the heritage of all men of science.

That which M. Comte proposed to do, was to give scientific thought and method a more definite embodiment and organization; and to apply it to the interpretation of classes of phenomena not previously dealt with in a scientific manner The conception was a great one; and the endeavour to work it out was worthy of sympathy and applause. Some such conception was entertained by Bacon. He, too, aimed at the organization of the sciences; he, too, held that "Physics is the mother of all the sciences;" he, too, held that the sciences can be advanced only by combining them,

and saw the nature of the required combination; he, too, held that moral and civil philosophy could not flourish when separated from their roots in natural philosophy; and thus he, too, had some idea of a social science growing out of physical science. But the state of knowledge in his day prevented any advance beyond the general conception: indeed, it was marvellous that he should have advanced so far. Instead of a vague, undefined conception, M. Comte has presented the world with a defined and highly-elaborated conception. In working out this conception he has shown remarkable breadth of view, great originality, immense fertility of thought, unusual powers of generalization. Considered apart from the question of its truth, his system of Positive Philosophy is a vast achievement. But after according to M. Comte high admiration for his conception, for his effort to realize it, and for the faculty he has shown in the effort to realize it, there remains the inquiry—Has he succeeded? A thinker who re-organizes the scientific method and knowledge of his age, and whose re-organization is accepted by his successors, may rightly be said to have such successors for his disciples. But successors who accept this method and knowledge of his age, *minus* his re-organization, are certainly not his disciples. How then stands the case with M. Comte? There are some few who receive his doctrines with but little reservation; and these are his disciples truly so called. There are others who regard with approval certain of his leading doctrines, but not the rest: these we may distinguish as partial adherents. There are others who reject all his distinctive doctrines; and these must be classed as his antagonists. The members of this class stand substantially in the same position as they would have done had he not written. Declining his re-organization of scientific doctrine, they possess this scientific doctrine in its pre-existing state, as the common heritage bequeathed by the past to the present; and their adhesion to

this scientific doctrine in no sense implicates them with M. Comte. In this class stand the great body of men of science. And in this class I stand myself.

Coming thus to the personal part of the question, let me first specify those great general principles on which M. Comte is at one with preceding thinkers: and on which I am at one with M. Comte.

All knowledge is from experience, holds M. Comte; and this I also hold—hold it, indeed, in a wider sense than M. Comte: since, not only do I believe that all the ideas acquired by individuals, and consequently all the ideas transmitted by past generations, are thus derived; but I also contend that the very faculties by which they are acquired, are the products of accumulated and organized experiences received by ancestral races of beings (see *Principles of Psychology*). But the doctrine that all knowledge is from experience, is not originated by M. Comte; nor is it claimed by him. He himself says—"Tous les bons esprits répètent, depuis Bacon, qu'il n'y a de connaissances réelle que celles qui reposent sur des faites observés." And the elaboration and definite establishment of this doctrine, has been the special characteristic of the English school of Psychology. Nor am I aware that M. Comte, accepting this doctrine, has done anything to make it more certain, or give it greater definiteness. Indeed it was impossible for him to do so; since he repudiates that part of mental science by which alone this doctrine can be proved.

It is a further belief of M. Comte, that all knowledge is phenomenal or relative; and in this belief I entirely agree. But no one alleges that the relativity of all knowledge was first enunciated by M. Comte. Among others who have more or less consistently held this truth, Sir William Hamilton enumerates, Protagoras, Aristotle, St. Augustin, Boethius, Averroes, Albertus Magnus, Gerson, Leo Hebræus, Melancthon, Scaliger, Francis Piccolomini, Giordano Bruno, Cam-

panella, Bacon, Spinoza, Newton, Kant. And Sir William Hamilton, in his "Philosophy of the Unconditioned," first published in 1829, has given a scientific demonstration of this belief. Receiving it in common with other thinkers, from preceding thinkers, M. Comte has not, to my knowledge, advanced this belief. Nor indeed could he advance it, for the reason already given—he denies the possibility of that analysis of thought which discloses the relativity of all cognition.

M. Comte reprobates the interpretation of different classes of phenomena by assigning metaphysical entities as their causes; and I coincide in the opinion that the assumption of such separate entities, though convenient, if not indeed necessary, for purposes of thought, is, scientifically considered, illegitimate. This opinion is, in fact, a corollary from the last; and must stand or fall with it. But like the last it has been held with more or less consistency for generations. M. Comte himself quotes Newton's favorite saying —"O! Physics, beware of Metaphysics!" Neither to this doctrine, any more than to the preceding doctrines, has M. Comte given a firmer basis. He has simply re-asserted it; and it was out of the question for him to do more. In this case, as in the others, his denial of subjective psychology debarred him from proving that these metaphysical entities are mere symbolic conceptions which do not admit of verification.

Lastly, M. Comte believes in invariable natural laws—absolute uniformities of relation among phenomena. But very many before him have believed in them too. Long familiar even beyond the bounds of the scientific world, the proposition that there is an unchanging order in things, has, within the scientific world, held, for generations, the position of an established postulate: by some men of science recognized only as holding of inorganic phenomena; but recognized by other men of science, as universal. And M. Comte, accepting this doctrine from the past, has left it substantially

as it was. Though he has asserted new uniformities, I do not think scientific men will admit that he has so *demonstrated* them, as to make the induction more certain; nor has he deductively established the doctrine, by showing that uniformity of relation is a necessary corollary from the persistence of force, as may readily be shown.

These, then, are the pre-established general truths with which M. Comte sets out—truths which cannot be regarded as distinctive of his philosophy. "But why," it will perhaps be asked, "is it needful to point out this; seeing that no instructed reader supposes these truths to be peculiar to M. Comte?" I reply that though no disciple of M. Comte would deliberately claim them for him; and though no theological antagonist at all familiar with science and philophy, supposes M. Comte to be the first propounder of them; yet there is so strong a tendency to associate any doctrines with the name of a conspicuous recent exponent of them, that false impressions are produced, even in spite of better knowledge. Of the need for making this reclamation, definite proof is at hand. In the No. of the *Revue des Deux Mondes* named at the commencement, may be found, on p. 936, the words—"Toute religion, comme toute philosophie, a la prétention de donner une explication de l'univers. La philosophie qui s'appelle *positive* se distingue de toutes les philosophies et de toutes les religions en ce qu'elle a renoncé à cette ambition de l'esprit humain;" and the remainder of the paragraph is devoted to explaining the doctrine of the relativity of knowledge. The next paragraph begins—"Tout imbu de ces idées, que nous exposons sans les discuter pour le moment, M. Spencer divise, etc." Now this is one of those collocations of ideas which tends to create, or to strengthen, the erroneous impression I would dissipate. I do not for a moment suppose that M. Laugel intended to say that these ideas which he describes as ideas of the "Positive Philosophy," are peculiarly the ideas of M. Comte. But

little as he probably intended it, his expressions suggest this conception. In the minds of both disciples and antagonists, "the Positive Philosophy" means the philosophy of M. Comte; and to be imbued with the ideas of "the Positive Philosophy" means to be imbued with the ideas of M. Comte—to have received these ideas from M. Comte. After what has been said above, I need scarcely repeat that the conception thus inadvertently suggested, is a wrong one. M. Comte's brief enunciations of these general truths, gave me no clearer apprehensions of them than I had before. Such clarifications of ideas on these ultimate questions, as I can trace to any particular teacher, I owe to Sir William Hamilton

From the principles which M. Comte held in common with many preceding and contemporary thinkers, let us pass now to the principles that are distinctive of his system. Just as entirely as I agree with M. Comte on those cardinal doctrines which we jointly inherit; so entirely do I disagree with him on those cardinal doctrines which he propounds, and which determine the organization of his philosophy. The best way of showing this will be to compare, side by side, the—

Propositions held by M. Comte.	*Propositions which I hold.*
"... chacune de nos conceptions principales, chaque branche de nos connaissances, passe successivement par trois états théoriques différens: l'état théologique, ou fictif; l'état métaphysique, ou abstrait; l'état scientifique, ou positif. En d'autres termes, l'esprit humain, par sa nature, emploie successivement dans chacune de ses recherches trôis méthodes de philoso-	The progress of our conceptions, and of each branch of knowledge, is from beginning to end intrinsically alike. There are not three methods of philosophizing radically opposed; but one method of philosophizing which remains, in essence, the same. At first, and to the last, the conceived causal agencies of phenomena, have a degree of generality corresponding to the width of the generalizations which experiences have determined; and they change just as gradually as experiences accumulate. The inte-

gration of causal agencies, originally thought of as multitudinous and local, but finally believed to be one and universal, is a process which involves the passing through all intermediate steps between these extremes; and any appearance of stages can be but superficial. Supposed concrete and individual causal agencies, coalesce in the mind as fast as groups of phenomena are assimilated, or seen to be similarly caused. Along with their coalescence, comes a greater extension of their individualities, and a concomitant loss of distinctness in their individualities. Gradually, by continuance of such coalescences, causal agencies become, in thought, diffused and indefinite. And eventually, without any change in the nature of the process, there is reached the consciousness of a universal causal agency, which cannot be conceived.*

As the progress of thought is one, so is the end one. There are not three possible terminal conceptions; but only a single terminal conception. When the theological idea of the providential action of one being, is developed to its ultimate form, by the absorption of all independent secondary agencies, it becomes the conception of a being immanent in all phenomena; and the reduction of it to this state, implies the fading-away, in thought, of all those anthropomorphic attributes by which the aboriginal

pher, dont le caractère est essentiellement différent et même radicalement opposé: d'abord la méthode théologique, ensuite la méthode métaphysique, et enfin la méthode positive." p. 3.

"Le système théologique est parvenu à la plus haute perfection dont il soit susceptible, quand il a substitué l'action providentielle d'un être unique au jeu varié des nombreuses divinités indépendantes qui avaient été imaginées primitivement. De même, le dernier terme du système metaphysique consiste à concevoir, au lieu des différentes entités particulières,

* A clear illustration of this process, is furnished by the recent mental integration of Heat, Light, Electricity, etc., as modes of molecular motion. If we go a step back, we see that the modern conception of Electricity, resulted from the integration in consciousness, of the two forms of it evolved in the galvanic battery and in the electric-machine. And going back to a still earlier stage, we see how the conception of statical electricity, arose by the coalescence in thought, of the previously-separate forces manifested in rubbed amber, in rubbed glass, and in lightning. With such illustrations before him, no one can, I think, doubt that the process has been the same from the beginning.

une seule grande entité générale, la *nature*, envisagée comme la source unique de tous les phénomènes. Pareillement, la perfection du système positif, vers laquelle il tend sans cesse, quoiqu'il soit très-probable qu'il ne doive jamais l'atteindre, serait de pouvoir se représenter tous les divers phénomènes observables comme des cas particuliers d'un seul fait général, tel que celui de la gravitation, par exemple." p. 5.

idea was distinguished. The alleged last term of the metaphysical system—the conception of a single great general entity, *nature*, as the source of all phenomena—is a conception identical with the previous one: the consciousness of a single source which, in coming to be regarded as universal, ceases to be regarded as conceivable, differs in nothing but name from the consciousness of one being, manifested in all phenomena. And similarly, that which is described as the ideal state of science—the power to represent all observable phenomena as particular cases of a single general fact, implies the postulating of some ultimate existence of which this single fact is alleged; and the postulating of this ultimate existence, involves a state of consciousness indistinguishable from the other two.

". . . la perfection du système positif, vers laquelle il tend sans cesse, quoiqu'il soit très-probable qu'il ne doive jamais l'atteindre, serait de pouvoir se représenter tous les divers phénomènes observables comme des cas particuliers d'un seul fait général. p. 5 . . . . . . considérant comme absolument inaccessible, et vide de sens pour nous la recherche de ce qu'on appelle les *causes*, soit premières, soit finales." p. 14.

Though along with the extension of generalizations, and concomitant integration of conceived causal agencies, the conceptions of causal agencies grow more indefinite; and though as they gradually coalesce into a universal causal agency, they cease to be representable in thought, and are no longer supposed to be comprehensible; yet the consciousness of *cause* remains as dominant to the last as it was at first; and can never be got rid of. The consciousness of cause can be abolished only by abolishing consciousness itself.* (*First Principles*, § 26.)

* Possibly it will be said that M. Comte himself admits, that what he calls the perfection of the positive system, will probably never be reached; and that what he condemns is the inquiry into the *natures* of causes and not the general recognition of cause. To the first of these allegations, I reply that, as I understand M. Comte, the obstacle to the perfect realization of the positive philosophy is the impossibility of carrying generalization so far as to reduce all particular facts to

'Ce n'est pas aux lecteurs de cet ouvrage que je croirai jamais devoir prouver que les idées gouvernent et bouleversent le monde, ou, en d'autres termes, que tout le mécanisme social repose finalement sur des opinions. Ils savent surtout que la grande crise politique et morale des sociétés actuelles tient, en dernière analyse, à l'anarchie intellectuelle." p. 48.*

Ideas do not govern and overthrow the world: the world is governed or overthrown by feelings, to which ideas serve only as guides. The social mechanism does not rest finally upon opinions; but almost wholly upon character. Not intellectual anarchy, but moral antagonism, is the cause of political crises. All social phenomena are produced by the totality of human emotions and beliefs: of which the emotions are mainly pre-determined, while the beliefs are mainly post-determined. Men's desires are chiefly inherited; but their beliefs are chiefly acquired, and depend on surrounding conditions; and the most important surrounding conditions depend on the social state which the prevalent desires have produced. The social state at any time existing, is the resultant of all the ambitions, self-interests, fears, reverences, indignations, sympathies, etc., of ancestral citizens and existing citizens. The ideas current in this social state, must, on the average, be congruous with the feelings of citizens; and therefore, on the average, with the social state these feelings have pro-

cases of one general fact—not the impossibility of excluding the consciousness of cause. And to the second allegation I reply, that the essential principle of his philosophy, is an avowed ignoring of cause altogether. For if it is not, *what becomes of his alleged distinction between the perfection of the positive system and the perfection of the metaphysical system?* And here let me point out that, by affirming exactly the opposite to that which M. Comte thus affirms, I am excluded from the positive school. If his own definition of positivism is to be taken, then, as I hold that what he defines as positivism is an absolute impossibility, it is clear that I cannot be what he calls a positivist.

* A friendly critic alleges that M. Comte is not fairly represented by this quotation, and that he is blamed by his biographer, M. Littré, for his too-great insistance on feeling as a motor of humanity. If in his "Positive Politics," which I presume is here referred to, M. Comte abandons his original position, so much the better. But I am here dealing with what is known as "the Positive Philosophy;" and that the passage above quoted does not misrepresent it, is proved by the fact that this doctrine is re-asserted at the commencement of the Sociology.

duced. Ideas wholly foreign to this social state cannot be evolved, and if introduced from without, cannot get accepted—or, if accepted, die out when the temporary phase of feeling which caused their acceptance, ends. Hence, though advanced ideas when once established, act upon society and aid its further advance; yet the establishment of such ideas depends on the fitness of the society for receiving them. Practically, the popular character and the social state, determine what ideas shall be current; instead of the current ideas determining the social state and the character. The modification of men's moral natures, caused by the continuous discipline of social life, which adapts them more and more to social relations, is therefore the chief proximate cause of social progress. (*Social Statics*, chap. xxx.)

"...Je ne dois pas négliger d'indiquer d'avance, comme une propriété essentielle de l'échelle encyclopédique que je vais proposer, sa conformité générale avec l'ensemble de l'histoire scientifique; en ce sens, que, malgré la simultanéité réelle et continue du développement des différentes sciences, celles qui seront classées comme antérieures seront, en effet, plus anciennes et constamment plus avancées que celles présentées comme postérieures." p. 84. . . . . "Cet ordre est déterminé par le degré de simplicité, ou, ce qui revient au même, par le degré de généralité des phénomènes." p. 87.

The order in which the generalizations of science are established, is determined by the frequency and impressiveness with which different classes of relations are repeated in conscious experience; and this depends, partly on *the directness with which personal welfare is affected;* partly on *the conspicuousness of one or both the phenomena between which a relation is to be perceived;* partly on *the absolute frequency with which the relations occur;* partly on their *relative frequency of occurrence;* partly on their *degree of simplicity;* and partly on their *degree of abstractness.* (*First Principles*, 1st ed., § 36; appended to this pamphlet.)

"En résultat définitif, la mathématique, l'astronomie, la physique, la chimie, la physiologie, et la physique sociale; telle est la formule enclyopédique qui, parmi le très-grand nombre de classifications que comportent les six sciences fondamentales, est seule logiquement conforme à la hiérarchie naturelle et invariable des phénomènes." p. 115.

The sciences as arranged in this succession specified by M. Comte, *do not* logically conform to the natural and invariable hierarchy of phenomena; and there is no serial order whatever in which they can be placed, which represents either their logical dependence or the dependence of phenomena. (See *Genesis of Science*, and foregoing Essay.)

"On conçoit, en effet, que l'étude rationelle de chaque science fondamentale exigeant la culture préalable de toutes celles qui la précèdent dans notre hiérarchie enclyopédique, n'a pu faire de progrès réels et prendre son véritable caractère, qu' après un grand développement des sciences antérieures relatives à des phénomènes plus généraux, plus abstraits, moins compliqués, et indépendans des autres. C'est donc dans cet ordre que la progression, quoique simultanée, a dû avoir lieu." p. 100.

The historical development of the sciences *has not* taken place in this serial order; nor in any other serial order. There is no "true *filiation* of the sciences." From the beginning, the abstract sciences, the abstract-concrete sciences, and the concrete sciences, have progressed together: the first solving problems which the second and third presented, and growing only by the solution of the problems; and the second similarly growing by joining the first in solving the problems of the third. All along there has been a continuous action and reaction between the three great classes of sciences—an advance from concrete facts to abstract facts, and then an application of such abstract facts to the analysis of new orders of concrete facts. (See *Genesis of Science.*)

Such then are the organizing principles of M. Comte's philosophy. Leaving out of his "*Exposition*" those pre-established general doctrines which are the common property of modern thinkers; these are the general doctrines which remain—these are the doctrines which fundamentally distinguish his system. From every one of them I dissent. To each proposition I oppose either a widely-different pro-

position, or a direct negation; and I not only do it now, but have done it from the time when I became acquainted with his writings. This rejection of his cardinal principles should, I think, alone suffice; but there are sundry other views of his, some of them largely characterizing his system, which I equally reject. Let us glance at them.

How organic beings have originated, is an inquiry which M. Comte deprecates as a useless speculation: asserting, as he does, that species are immutable.	This inquiry, I believe, admits of answer, and will be answered. That division of Biology which concerns itself with the origin of species, I hold to be the supreme division, to which all others are subsidiary. For on the verdict of Biology on this matter, must wholly depend our conception of human nature, past, present, and future; our theory of the mind; and our theory of society.
M. Comte contends that of what is commonly known as mental science, all that most important part which consists of the subjective analysis of our ideas, is an impossibility.	I have very emphatically expressed my belief in a subjective science of the mind, by writing a *Principles of Psychology*, one half of which is subjective.
M. Comte's ideal of society is one in which *government* is developed to the greatest extent—in which class-functions are far more under conscious public regulation than now—in which hierarchical organization with unquestioned authority shall guide everything—in which the individual life shall be subordinated in the greatest degree to the social life.	That form of society towards which we are progressing, I hold to be one in which *government* will be reduced to the smallest amount possible, and *freedom* increased to the greatest amount possible — one in which human nature will have become so moulded by social discipline into fitness for the social state, that it will need little external restraint, but will be self-restrained—one in which the citizen will tolerate no interference with his freedom, save that which maintains the equal freedom of others—one in which the spontaneous co-operation which has developed our industrial system, and is now develop-

ing it with increasing rapidity, will produce agencies for the discharge of nearly all social functions, and will leave to the primary govermental agency nothing beyond the function of maintaining those conditions to free action, which make such spontaneous co-operation possible—one in which individual life will thus be pushed to the greatest extent consistent with social life; and in which social life will have no other end than to maintain the completest sphere for individual life.

M. Comte, not including in his philosophy the consciousness of a cause manifested to us in all phenomena, and yet holding that there must be a religion, which must have an object, takes for his object—Humanity. "This Collective Life (of Society) is in Comte's system the *Être Suprême;* the only one we can *know*, therefore the only one we can worship."

I conceive, on the other hand, that the object of religious sentiment will ever continue to be, that which it has ever been—the unknown source of things. While the *forms* under which men are conscious of the unknown source of things, may fade away, the *substance* of the consciousness is permanent. Beginning with causal agents conceived as imperfectly known; progressing to causal agents conceived as less known and less knowable; and coming at last to a universal causal agent posited as not to be known at all; the religious sentiment must ever continue to occupy itself with this universal causal agent. Having in the course of evolution, come to have for its object of contemplation, the Infinite Unknowable, the religious sentiment can never again (unless by retrogression) take a Finite Knowable, like Humanity, for its object of contemplation.

Here, then, are sundry other points, all of them important, and the last two supremely important, on which I am diametrically opposed to M. Comte; and did space permit, I could add many others. Radically differing from him as I thus do, in everything distinctive of his philosophy; and

having invariably expressed my dissent, publicly and privately, from the time I became acquainted with his writings; it may be imagined that I have been not a little startled to find myself classed as one of the same school. That those who have read *First Principles* only, may have been betrayed into this error in the way above shown, by the ambiguous use of the phrase "Positive Philosophy," I can understand. But that any who are acquainted with my previous writings, should suppose I have any general sympathy with M. Comte, save that implied by preferring proved facts to superstitions, astonishes me.

It is true that, disagreeing with M. Comte, though I do, in all those fundamental views that are peculiar to him, I agree with him in sundry minor views. The doctrine that the education of the individual should accord in mode and arrangement with the education of mankind, considered historically, I have cited from him; and have endeavoured to enforce it. I entirely concur in his opinion that there requires a new order of scientific men, whose function shall be that of co-ordinating the results arrived at by the rest. To him I believe I am indebted for the conception of a social *consensus*; and when the time comes for dealing with this conception, I shall state my indebtedness. And I also adopt his word, Sociology. There are, I believe, in the part of his writings which I have read, various incidental thoughts of great depth and value; and I doubt not that were I to read more of his writings, I should find many others.* It is very probable, too, that I have said (as I am told I have) some things which M. Comte had already said. It would be difficult, I believe, to find any two men who had no opinions in common. And it would be extremely strange if two men,

* M. Comte's "Exposition" I read in the original in 1853; and in two or three other places have referred to the original to get his exact words. The Inorganic Physics, and the first chapter of the Biology, I read in Miss Martineau's condensed translation, when it appeared. The rest of M. Comte's views I know only through Mr. Lewes's outline, and through incidental references.

starting from the same general doctrines established by modern science, should traverse some of the same fields of inquiry, without their lines of thought having any points of intersection. But none of these minor agreements can be of much weight in comparison with the fundamental disagreements above specified. Leaving out of view that general community which we both have with the scientific thought of the age, the differences between us are essential, while the correspondences are non-essential. And I venture to think that kinship must be determined by essentials, and not by non-essentials.*

Joined with the ambiguous use of the phrase "Positive Philosophy," which has led to a classing with M. Comte of many men who either ignore or reject his distinctive principles, there has been one special circumstance that has tended to originate and maintain this classing in my own case. The assumption of some relationship between M. Comte and myself, was unavoidably raised by the title of my first book—*Social Statics.* When that book was published, I was unaware that this title had been before used: had I known the fact, I should certainly have adopted an alternative title which I had in view.† If, however, instead of the title,

* In his recent work, *Auguste Comte et la Philosophie Positive,* M. Littré, defending the Comtean classification of the sciences from the criticism I made upon it in the "Genesis of Science," deals with me wholly as an antagonist. The chapter he devotes to his reply, opens by placing me in direct antithesis to the English adherents of Comte, named in the preceding chapter.

† I believed at the time, and have never doubted until now, that the choice of this title was absolutely independent of its previous use by M Comte. While writing these pages, I have found reason to think the contrary. On referring to *Social Statics,* to see what were my views of social evolution in 1850, when M. Comte was to me but a name, I met with the following sentence:—"Social philosophy may be aptly divided (as political economy has been) into statics and dynamics." (p. 409). This I remembered to be a reference to a division which I had seen in the Political Economy of Mr. Mill. But why had I not mentioned Mr. Mill's name? On referring to the first edition of his work, I found, at the opening of Book iv., this sentence:—"The three preceding parts include as detailed a view as the limits of this treatise permit, of what, by a happy generalization of a mathematical phrase, has been called the Statics of the subject." Here was the solution of the question. The division had not been made by Mr. Mill, but by some writer (on Political Economy I supposed) who was not named by him; and whom I did not know. It is now manifest, however, that while I supposed I was giving a more extended use to this division, I was but returning to the original use

the work itself be considered, its irrelation to the philosophy of M. Comte, becomes abundantly manifest. There is decisive testimony on this point. In the *North British Review* for August, 1851, a reviewer of *Social Statics* says—

"The title of this work, however, is a complete misnomer. According to all analogy, the phrase "Social Statics" should be used only in some such sense as that in which, as we have already explained, it is used by Comte, namely as designating a branch of inquiry whose end it is to ascertain the laws of social equilibrium or order, as distinct ideally from those of social movement or progress. Of this Mr. Spencer does not seem to have had the slightest notion, but to have chosen the name for his work only as a means of indicating vaguely that it proposed to treat of social concerns in a scientific manner." p. 321.

Respecting M. Comte's application of the words *statics* and *dynamics* to social phenomena, now that I know what it is, I will only say that while I perfectly understand how, by a defensible extension of their mathematical meanings, the one may be used to indicate social *functions in balance*, and the other social *functions out of balance*, I am quite at a loss to understand how the phenomena of *structure* can be included in the one any more than in the other. But the two things which here concern me, are, first, to point out that I had not "the slightest notion" of giving Social Statics the meaning which M. Comte gave it; and, second, to explain the meaning which I did give it. The units of any aggregate of matter, are in equilibrium when they severally act and re-act upon each other on all sides with equal forces. A state of change among them implies that there are forces exercised by some that are not counterbalanced by like forces exercised by others; and a state of rest implies the absence of such uncounterbalanced forces—implies, if the units are homogeneous, equal distances among them—implies a maintenance of their respective spheres of molecular

which Mr. Mill had limited to his special topic. Another thing is, I think, tolerably manifest. As I evidently wished to point out my obligation to some unknown political economist, whose division I thought I was extending, I should have named him had I known who he was. And in that case should not have put this extension of the division as though it were new

motion. Similarly among the units of a society, the fundamental condition to equilibrium, is, that the restraining forces which the units exercise on each other, shall be balanced. If the spheres of action of some units are diminished by extension of the spheres of action of others, there necessarily results an unbalanced force which tends to produce political change in the relations of individuals; and the tendency to change can cease, only when individuals cease to aggress on each other's spheres of action—only when there is maintained that law of equal freedom, which it was the purpose of *Social Statics* to enforce in all its consequences. Besides this totally-unlike conception of what constitutes Social Statics, the work to which I applied that title, is fundamentally at variance with M. Comte's teachings in almost everything. So far from alleging, as M. Comte does, that society is to be re-organized by philosophy; it alleges that society is to be re-organized only by the accumulated effects of habit on character. Its aim is not the increase of authoritative control over citizens, but the decrease of it. A more pronounced individualism, instead of a more pronounced nationalism, is its ideal. So profoundly is my political creed at variance with the creed of M. Comte, that, unless I am misinformed, it has been instanced by a leading English disciple of M. Comte, as the creed to which he has the greatest aversion. One point of coincidence, however, is recognizable. The analogy between an individual organism and a social organism, which was held by Plato and by Hobbes, is asserted in *Social Statics*, as it is in the *Sociology* of M. Comte. Very rightly, M. Comte has made this analogy the cardinal idea of this division of his philosophy. In *Social Statics*, the aim of which is essentially ethical, this analogy is pointed out incidentally, to enforce certain ethical considerations; and is there obviously suggested partly by the definition of life which Coleridge derived from Schelling, and partly by the generalizations of physiologists there referred to (chap. xxx. §§. 12, 13, 16). Excepting

this incidental agreement, however, the contents of *Social Statics* are so wholly antagonistic to the philosophy of M. Comte, that, but for the title, the work would never, I think, have raised the remembrance of him—unless, indeed, by the association of opposites.*

And now let me point out that which really *has* exercised a profound influence over my course of thought. The truth which Harvey's embryological inquiries first dimly indicated, which was afterwards more clearly perceived by Wolff, and which was put into a definite shape by Von Baer—the truth that all organic development is a change from a state of homogeneity to a state of heterogeneity—this it is from which very many of the conclusions which I now hold, have indirectly resulted. In *Social Statics*, there is everywhere manifested a dominant belief in the evolution of man and of society. There is also manifested the belief that this evolution is in both cases determined by the incidence of conditions—the actions of circumstances. And there is further, in the sections above referred to, a recognition of the fact that organic and social evolutions, conform to the same law. Falling amid beliefs in evolutions of various orders, everywhere determined by natural causes (beliefs again displayed in the *Theory of Population* and in the *Principles of Psychology*); the formula of Von Baer acted as an organizing principle. The extension of it to other kinds of phenomena than those of individual and social organiza-

* Let me add that the conception developed in *Social Statics*, dates back to a series of letters on the "Proper Sphere of Government," published in the *Nonconformist* newspaper, in the latter half of 1842, and republished as a pamphlet in 1843. In these letters will be found, along with many crude ideas, the same belief in the conformity of social phenomena to unvariable laws; the same belief in human progression as determined by such laws; the same belief in the moral modification of men as caused by social discipline; the same belief in the tendency of social arrangements "of themselves to assume a condition of *stable* equilibrium;" the same repudiation of state-control over various departments of social life; the same limitation of state-action to the maintenance of equitable relations among citizens. The writing of *Social Statics* arose from a dissatisfaction with the basis on which the doctrines set forth in those letters were placed: the second half of that work is an elaboration of these doctrines; and the first half a statement of the principles from which they are deducible.

tion, is traceable through successive stages. It may be seen in the last paragraph of an essay on "The Philosophy of Style," published in October, 1852; again in an essay on "Manners and Fashion," published in April, 1854; and then, in a comparatively advanced form, in an essay on "Progess: its Law and Cause," published in April, 1857. Afterwards, there came the recognition of the need for further limitation of this formula; next the inquiry into those general laws of force from which this universal transformation necessarily results; next the deduction of these from the ultimate law of the persistence of force; next the perception that there is everywhere a process of Dissolution complementary to that of Evolution; and, finally, the determination of the conditions (specified in the foregoing essay) under which Evolution and Dissolution respectively occur. The filiation of these results, is, I think, tolerably manifest. The process has been one of continuous development, set up by the addition of Von Baer's law to a number of ideas that were in harmony with it. And I am not conscious of any other influences by which the process has been affected.

It is possible, however, that there may have been influences of which I am not conscious; and my opposition to M. Comte's system may have been one of them. The presentation of antagonistic thoughts, often produces greater definiteness and development of one's own thoughts. It is probable that the doctrines set forth in the essay on "The Genesis of Science," might never have been reached, had not my very decided dissent from M. Comte's conception, led me to work them out; and but for this, I might not have arrived at the classification of the sciences exhibited in the foregoing essay. Very possibly there are other cases in which the stimulus of repugnance to M. Comte's views, may have aided in elaborating my own views; though I cannot call to mind any other cases.

Let it by no means be supposed from all I have said, that I do not regard M. Comte's speculations as of great value.

True or untrue, his system as a whole, has doubtless produced important and salutary revolutions of thought in many minds; and will doubtless do so in many more. Doubtless, too, not a few of those who dissent from his general views, have been healthfully stimulated by consideration of them. The presentation of scientific knowledge and method as a whole, whether rightly or wrongly co-ordinated, cannot have failed greatly to widen the conceptions of most of his readers. And he has done especial service by familiarizing men with the idea of a social science, based on the other sciences. Beyond which benefits resulting from the general character and scope of his philosophy, I believe that there are scattered through his pages, many large ideas that are valuable not only as stimuli, but for their actual truth.

It has been by no means an agreeable task to make these personal explanations; but it has seemed to me a task not to be avoided. Differing so profoundly as I do from M. Comte on all fundamental doctrines, save those which we inherit in common from the past; it has become needful to dissipate the impression that I agree with him—needful to show that a large part of what is currently known as "positive philosophy," is not "positive philosophy" in the sense of being peculiarly M. Comte's philosophy; and to show that beyond that portion of the so-called "positive philosophy" which is not peculiar to him, I dissent from it.

And now at the close, as at the outset, let me express my great regret that these explanations should have been called forth by the statements of a critic who has treated me so liberally. Nothing will, I fear, prevent the foregoing pages from appearing like a very ungracious response to M. Laugel's sympathetically-written review. I can only hope that the gravity of the question at issue, in so far as it concerns myself, may be taken in mitigation, if not as a sufficient apology.

March 12th, 1864.

III.

OF LAWS IN GENERAL, AND THE ORDER OF THEIR DISCOVERY.

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[*The following chapter was contained in the first edition of* First Principles. *I omitted it from the re-organized second edition, because it did not form an essential part of the new structure. As it is referred to in the foregoing pages, and as its general argument is germane to the contents of those pages, I have thought well to append it here. Moreover, though I hope eventually to incorporate it in that division of the* Principles of Sociology *which treats of Intellectual Progress, yet as it must be long before it can thus re-appear in its permanent place, and as, should I not get so far in the execution of my undertaking, it may never thus re-appear at all, it seems proper to make it more accessible than it is at present. The first and last sections, which served to link it into the argument of the work to which it originally belonged, are omitted. The rest has been carefully revised, and in some parts considerably altered.*]

The recognition of Law being the recognition of uniformity of relations among phenomena, it follows that the order in which different groups of phenomena are reduced to law, must depend on the frequency with which the uniform relations they severally display are distinctly experienced. At any given stage of progress, those uniformities will be best known with which men's minds have been oftenest and most strongly impressed. In proportion partly to the number of times a relation has been presented to consciousness (not merely to the senses), and in proportion

partly to the vividness with which the terms of the relation have been cognized, will be the degree in which the constancy of connexion is perceived.

The succession in which relations are generalized being thus determined, there result certain derivative principles to which this succession must more immediately and obviously conform. First is *the directness with which personal welfare is affected.* While, among surrounding things, many do not appreciably influence us in any way, some produce pleasures and some pains, in various degrees; and manifestly, those things whose actions on the organism for good or evil are most decided, will, cæteris paribus, be those whose laws of action are earliest observed. Second comes *the conspicuousness of one or both phenomena between which a relation is to be perceived.* On every side are phenomena so concealed as to be detected only by close observation; others not obtrusive enough to attract notice; others which moderately solicit the attention; others so imposing or vivid as to force themselves on consciousness; and, supposing conditions to be the same, these last will of course be among the first to have their relations generalized. In the third place, we have *the absolute frequency with which the relations occur.* There are coexistences and sequences of all degrees of commonness, from those which are ever present to those which are extremely rare; and manifestly, the rare coexistences and sequences, as well as the sequences which are very long in taking place, will not be reduced to law so soon as those which are familiar and rapid. Fourthly has to be added *the relative frequency of occurrence.* Many events and appearances are limited to certain times or certain places, or both; and, as a relation which does not exist within the environment of an observer cannot be perceived by him, however common it may be elsewhere or in another age, we have to take account of the surrounding physical circum-

stances, as well as of the state of society, of the arts, and of the sciences—all of which affect the frequency with which certain groups of facts are observable. The fifth corollary to be noticed is, that the succession in which different classes of relations are reduced to law, depends in part on their *simplicity*. Phenomena presenting great composition of causes or conditions, have their essential relations so masked, that it requires accumulated experiences to impress upon consciousness the true connexions of antecedents and consequents they involve. Hence, other things equal, the progress of generalization will be from the simple to the complex; and this it is which M. Comte has wrongly asserted to be the sole regulative principle of the progress. Sixth comes *the degree of abstractness*. Concrete relations are the earliest acquisitions. Such analyses of them as separate the essential connexions from their disguising accompaniments, necessarily come later. The analyses of the connexions, always more or less compound, into their elements then becomes possible. And so on continually, until the highest and most abstract truths have been reached.

These, then, are the several derivative principles. The frequency and vividness with which uniform relations are repeated in conscious experience, determining the recognition of their uniformity, and this frequency and vividness depending on the above conditions, it follows that the order in which different classes of facts are generalized, must depend on the extent to which the above conditions are fulfilled in each class. Let us mark how the facts harmonize with this conclusion: taking first a few that elucidate the general truth, and afterwards some that exemplify the special truths which we here see follow from it.

The relations earliest known as uniformities, are those subsisting between the common properties of matter—tangi-

bility, visibility, cohesion, weight, etc. We have no trace of a time when the resistance offered by an object was regarded as caused by the will of the object; or when the pressure of a body on the hand holding it, was ascribed to the agency of a living being. And accordingly, these are the relations of which we are oftenest conscious; being objectively frequent, conspicuous, simple, concrete, and of immediate personal concern.

Similarly with the ordinary phenomena of motion. The fall of a mass on the withdrawal of its support, is a sequence which directly affects bodily welfare, is conspicuous, simple, concrete, and very often repeated. Hence it is one of the uniformities recognized before the dawn of tradition. We know of no era when movements due to terrestrial gravitation were attributed to volition. Only when the relation is obscured—only, as in the case of an aërolite, where the antecedent of the descent is unperceived, do we find the conception of personal agency. On the other hand, motions of intrinsically the same order as that of a falling stone—those of the heavenly bodies—long remain ungeneralized; and until their uniformity is seen, are construed as results of will. This difference is clearly not dependent on comparative complexity or abstractness; since the motion of a planet in an ellipse, is as simple and concrete a phenomenon as the motion of a projected arrow in a parabola. But the antecedents are not conspicuous; the sequences are of long duration; and they are not often repeated. And that these are the causes of their slow reduction to law, we see in the fact that they are severally generalized in the order of their frequency and conspicuousness—the moon's monthly cycle, the sun's annual change, the periods of the inferior planets, the periods of the superior planets.

While astronomical sequences were still ascribed to volition, certain terrestrial sequences of a different kind, but some of them equally without complication, were interpreted in like manner. The solidification of water at a low tempe-

rature, is a phenomenon that is simple, concrete, and of much personal concern. But it is neither so frequent as those which we see are earliest generalized, nor is the presence of the antecedent so manifest. Though in all but tropical climates, mid-winter displays the relation between cold and freezing with tolerable constancy; yet, during the spring and autumn, the occasional appearance of ice in the mornings has no very obvious connexion with coldness of the weather. Sensation being so inaccurate a measure, it is not possible for the savage to experience the definite relation between a temperature of 32° and the congealing of water; and hence the long continued belief in personal agency. Similarly, but still more clearly, with the winds. The absence of regularity and the inconspicuousness of the antecedents, allowed the mythological explanation to survive for a great period.

During the era in which the uniformity of many quite simple inorganic relations was still unrecognized, certain organic relations, intrinsically very complex and special, were generalized. The constant coexistence of feathers and a beak, of four legs with an internal bony framework, are facts which were, and are, familiar to every savage. Did a savage find a bird with teeth, or a mammal clothed with feathers, he would be as much surprised as an instructed naturalist. Now these uniformities of organic structure thus early perceived, are of exactly the same kind as those more numerous ones later established by biology. The constant coexistence of mammary glands with two occipital condyles to the skull, of vertebræ with teeth lodged in sockets, of frontal horns with the habit of rumination, are generalizations as purely empirical as those known to the aboriginal hunter. The botanist cannot in the least understand the complex relation between papilionaceous flowers and seeds borne in flattened pods: he knows these and like connexions simply in the same way that the barbarian knows the con-

nexions between particular leaves and particular kinds of wood. But the fact that sundry of the uniform relations which chiefly make up the organic sciences, were very early recognized, is due to the high degree of vividness and frequency with which they were presented to consciousness. Though the connexion between the sounds characteristic of a bird, and the possession of edible flesh, is extremely involved; yet the two terms of the relation are conspicuous, often recur in experience, and a knowledge of their connexion has a direct bearing on personal welfare. Meanwhile innumerable relations of the same order, which are displayed with even greater frequency by surrounding plants and animals, remain for thousands of years unrecognised, if they are unobtrusive or of no apparent moment.

When, passing from this primitive stage to a more advanced stage, we trace the discovery of those less familiar uniformities which mainly constitute what is distinguished as Science, we find the succession in which knowledge of them is reached, to be still determined in the same manner. This will become obvious on contemplating separately the influence of each derivative condition.

How relations that have immediate bearings on the maintenance of life, are, other things equal, fixed in the mind before those which have no immediate bearings, the history of Science abundantly illustrates. The habits of existing uncivilized races, who fix times by moons and barter so many of one article for so many of another, show us that conceptions of equality and number, which are the germs of mathematical science, were developed under the immediate pressure of personal wants; and it can scarcely be doubted that those laws of numerical relations which are embodied in the rules of arithmetic, were first brought to light through the practice of mercantile exchange. Similarly with geometry. The derivation of the word shows us that it ori-

ginally included only certain methods of partitioning ground and laying out buildings. The properties of the scales and the lever, involving the first principle in mechanics, were early generalized under the stimulus of commercial and architectural needs. To fix the times of religious festivals and agricultural operations, were the motives which led to the establishment of the simpler astronomic periods. Such small knowledge of chemical relations as was involved in ancient metallurgy, was manifestly obtained in seeking how to improve tools and weapons. In the alchemy of later times, we see how greatly an intense hope of private benefit contributed to the disclosure of a certain class of uniformities. Nor is our own age barren of illustrations. "Here," says Humboldt, when in Guiana, "as in many parts in Europe, the sciences are thought worthy to occupy the mind, only so far as they confer some immediate and practical benefit on society." "How is it possible to believe," said a missionary to him. "that you have left your country to come to be devoured by mosquitoes on this river, and to measure lands that are not your own." Our coasts furnish like instances. Every sea-side naturalist knows how great is the contempt with which fishermen regard the collection of objects for the microscope or aquarium. Their incredulity as to the possible value of such things is so great, that they can scarcely be induced even by bribes to preserve the refuse of their nets. Nay, we need not go for evidence beyond daily table-talk. The demand for "practical science"—for a knowledge that can be brought to bear on the business of life—joined to the ridicule commonly vented on scientific pursuits having no obvious uses, suffice to show that the order in which laws are discovered greatly depends on the directness with which they affect our welfare.

That, when all other conditions are the same, obtrusive relations will be generalized before unobtrusive ones, is so nearly a truism that examples appear almost superfluous. If

it be admitted that by the aboriginal man, as by the child, the co-existent properties of large surrounding objects are noticed before those of minute objects, and that the external relations which bodies present are generalized before their internal relations, it must be admitted that in subsequent stages of progress, the comparative conspicuousness of relations has greatly affected the order in which they were recognized as uniform. Hence it happened that after the establishment of those very manifest sequences constituting a lunation, and those less manifest ones marking a year, and those still less manifest ones marking the planetary periods, astronomy occupied itself with such inconspicuous sequences as those displayed in the repeating cycle of lunar eclipses, and those which suggested the theory of epicycles and eccentrics; while modern astronomy deals with still more inconspicuous sequences, some of which, as the planetary rotations, are nevertheless the simplest which the heavens present. In physics, the early use of canoes implied an empirical knowledge of certain hydrostatic relations that are intrinsically more complex than sundry static relations not empirically known; but these hydrostatic relations were thrust upon observation. Or, if we compare the solution of the problem of specific gravity by Archimedes with the discovery of atmospheric pressure by Torricelli (the two involving mechanical relations of exactly the same kind), we perceive that the much earlier occurrence of the first than the last was determined, neither by a difference in the irbearings on personal welfare, nor by a difference in the frequency with which illustrations of them came under observation, nor by relative simplicity; but by the greater obtrusiveness of the connexion between antecedent and consequent in the one case than in the other. Among miscellaneous illustrations, it may be pointed out that the connexions between lightning and thunder, and between rain and clouds, were recognized long before others of the same order, simply because they

thrust themselves on the attention. Or the long-delayed discovery of the microscopic forms of life, with all the phenomena they present, may be named as very clearly showing how certain groups of relations not ordinarily perceptible, though in other respects like long-familiar relations, have to wait until changed conditions render them perceptible. But, without further details, it needs only to consider the inquiries which now occupy the electrician, the chemist, the physiologist, to see that science has advanced, and is advancing, from the more conspicuous phenomena to the less conspicuous ones.

How the degree of absolute frequency of a relation affects the recognition of its uniformity, we see in contrasting certain biological facts. The connexion between death and bodily injury, constantly displayed not only in men but in all inferior creatures, was known as an instance of natural causation while yet deaths from diseases were thought supernatural. Among diseases themselves, it is observable that unusual ones were regarded as of demoniacal origin during ages when the more frequent were ascribed to ordinary causes: a truth paralleled among our own peasantry, who by the use of charms show a lingering superstition with respect to rare disorders, which they do not show with respect to common ones, such as colds. Passing to physical illustrations, we may note that within the historic period whirlpools were accounted for by the agency of water-spirits; but we do not find that within the same period the disappearance of water on exposure either to the sun or to artificial heat was interpreted in an analogous way: though a more marvellous occurrence, and a much more complex one, its great frequency led to the early recognition of it as a natural uniformity. Rainbows and comets do not differ much in conspicuousness, and a rainbow is intrinsically the more involved phenomenon; but chiefly because of their far greater commonness, rainbows were perceived to have a direct dependence

on sun and rain while yet comets were regarded as signs of divine wrath.

That races living inland must long have remained ignorant of the daily and monthly sequences of the tides, and that tropical races could not early have comprehended the phenomena of northern winters, are extreme illustrations of the influence which relative frequency has on the recognition of uniformities. Animals which, where they are indigenous, call forth no surprise by their structures or habits, because these are so familiar, when taken to countries where they have never been seen, are looked at with an astonishment approaching to awe—are even thought supernatural: a fact which will suggest numerous others that show how the localization of phenomena in part controls the order in which they are reduced to law. Not only however does their localization in space affect the progression, but also their localization in time. Facts which are rarely if ever manifested in one era, are rendered very frequent in another, simply through the changes wrought by civilization. The lever, of which the properties are illustrated in the use of sticks and weapons, is vaguely understood by every savage—on applying it in a certain way he rightly anticipates certain effects; but the wheel-and-axle, pulley, and screw, cannot have their powers either empirically or rationally known till the advance of the arts has more or less familiarized them. Through those various means of exploration which we have inherited and added to, we have become acquainted with a vast range of chemical relations that were relatively non-existent to the primitive man. To highly-developed industries we owe both the substances and the appliances that have disclosed to us countless uniformities which our ancestors had no opportunity of seeing. These and like instances that will occur to the reader, show that the accumulated materials, and processes, and products, which characterize the environments of complex societies, greatly increase the accessibility of various

classes of relations; and by so multiplying the experiences of them, or making them relatively frequent, facilitate their generalization. Moreover, various classes of phenomena presented by society itself, as for instance those which political economy formulates, become relatively frequent, and therefore recognizable, in advanced social states; while in less advanced ones they are either too rarely displayed to have their relations perceived, or, as in the least advanced ones, are not displayed at all.

That, where no other circumstances interfere, the order in which different uniformities are established varies as their complexity, is manifest. The geometry of straight lines was understood before the geometry of curved lines; the properties of the circle before the properties of the ellipse, parabola, and hyperbola; and the equations of curves of single curvature were ascertained before those of curves of double curvature. Plane trigonometry comes in order of time and simplicity before spherical trigonometry; and the mensuration of plane surfaces and solids before the mensuration of curved surfaces and solids. Similarly with mechanics: the laws of simple motion were generalized before those of compound motion; and those of rectilinear motion before those of curvilinear motion. The properties of equal-armed levers or scales, were understood before those of levers with unequal arms; and the law of the inclined plane was formulated earlier than that of the screw, which involves it. In chemistry, the progress has been from the simple inorganic compounds to the more involved or organic compounds. And where, as in the higher sciences, the conditions of the exploration are more complicated, we still may clearly trace relative complexity as determining the order of discovery where other things are equal.

The progression from concrete relations to abstract ones, and from the less abstract to the more abstract, is equally obvious. Numeration, which in its primary form concerned

itself only with groups of actual objects, came earlier than simple arithmetic; the rules of which deal with numbers apart from objects. Arithmetic, limited in its sphere to concrete numerical relations, is alike earlier and less abstract than Algebra, which deals with the relations of these relations. And in like manner, the Calculus of Operations comes after Algebra, both in order of evolution and in order of abstractness. In Mechanics, the more concrete relations of forces exhibited in the lever, inclined plane, etc., were understood before the more abstract relations expressed in the laws of resolution and composition of forces; and later than the three abstract laws of motion as formulated by Newton came the still more abstract law of inertia. Similarly with Physics and Chemistry, there has been an advance from truths entangled in all the specialities of particular facts and particular classes of facts, to truths disentangled from the disguising incidents under which they are manifested—to truths of a higher abstractness.

Brief and rude as is this sketch of a mental development that has been long and complicated, I venture to think it shows inductively what was deductively inferred, that the order in which separate groups of uniformities are recognized, depends not on one circumstance but on several circumstances. The various classes of relations are generalized in a certain succession, not solely because of one particular kind of difference in their natures; but also because they are variously placed in time and in space, variously open to observation, and variously related to our own constitutions: our perception of them being influenced by all these conditions in endless combinations. The comparative degrees of importance, of obtrusiveness, of absolute frequency, of relative frequency, of simplicity, of concreteness, are every one of them factors; and from their unions in proportions that are never twice alike, there results a highly complex process of mental evolution. But while it is thus manifest

that the proximate causes of the succession in which relations are reduced to law, are numerous and involved; it is also manifest that there is one ultimate cause to which these proximate causes are subordinate. As the several circumstances that determine the early or late recognition of uniformities are circumstances that determine the number and strength of the impressions which these uniformities make on the mind, it follows that the progression conforms to a certain fundamental principle of psychology. We see *à posteriori*, what we concluded *à priori*, that the order in which relations are generalized, depends on the frequency and impressiveness with which they are repeated in conscious experience.

Having roughly analyzed the progress of the past, let us take advantage of the light thus thrown on the present, and consider what is implied respecting the future.

Note first that the likelihood of the universality of Law has been ever growing greater. Out of the countless coexistences and sequences with which mankind are environed, they have been continually transferring some from the group whose order was supposed to be arbitrary, to the group whose order is known to be uniform. And manifestly, as fast as the relations that are unreduced to law become fewer, the probability that among them there are some that do not conform to law, becomes less. To put the argument numerically—It is clear that when out of surrounding phenomena a hundred of several kinds have been found to occur in constant connexions, there arises a slight presumption that all phenomena occur in constant connexions. When uniformity has been established in a thousand cases, more varied in their kinds, the presumption gains strength. And when the known cases of uniformity amount to myriads, including many of each variety, it becomes an ordinary induction that uniformity exists everywhere.

Silently and insensibly their experiences have been pressing men on towards the conclusion thus drawn. Not out of a conscious regard for these reasons, but from a habit of thought which these reasons formulate and justify, all minds have been advancing towards a belief in the constancy of surrounding coexistences and sequences. Familiarity with concrete uniformities has generated the abstract conception of uniformity—the idea of *Law;* and this idea has been in successive generations slowly gaining fixity and clearness. Especially has it been thus among those whose knowledge of natural phenomena is the most extensive—men of science. The mathematician, the physicist, the astronomer, the chemist, severally acquainted with the vast accumulations of uniformities established by their predecessors, and themselves daily adding new ones as well as verifying the old, acquire a far stronger faith in law than is ordinarily possessed. With them this faith, ceasing to be merely passive, becomes an active stimulus to inquiry. Wherever there exist phenomena of which the dependence is not yet ascertained, these most cultivated intellects, impelled by the conviction that here too there is some invariable connexion, proceed to observe, compare, and experiment; and when they discover the law to which the phenomena conform, as they eventually do, their general belief in the universality of law is further strengthened. So overwhelming is the evidence, and such the effect of this discipline, that to the advanced student of nature, the proposition that there are lawless phenomena has become not only incredible but almost inconceivable.

This habitual recognition of law which already distinguishes modern thought from ancient thought, must spread among men at large. The fulfilment of predictions made possible by every new step, and the further command gained of nature's forces, prove to the uninitiated the validity of scientific generalizations and the doctrine they illustrate. Widening education is daily diffusing among the mass of

men that knowledge of these generalizations which has been hitherto confined to the few. And as fast as this diffusion goes on, must the belief of the scientific become the belief of the world at large.

That law is universal, will become an irresistible conclusion when it is perceived that *the progress in the discovery of laws itself conforms to law*; and when this perception makes it clear why certain groups of phenomena have been reduced to law, while other groups are still unreduced. When it is seen that the order in which uniformities are recognized, must depend upon the frequency and vividness with which they are repeated in conscious experience; when it is seen that, as a matter of fact, the most common, important, conspicuous, concrete, and simple, uniformities were the earliest recognized, because they were experienced oftenest and most distinctly; it will by implication be seen that long after the great mass of phenomena have been generalized, there must remain phenomena which, from their rareness, or unobtrusiveness, or seeming unimportance, or complexity, or abstractness, are still ungeneralized. Thus will be furnished a solution to a difficulty sometimes raised. When it is asked why the universality of law is not already fully established, there will be the answer that the directions in which it is not yet established are those in which its establishment must necessarily be latest. That state of things which is inferable beforehand, is just the state which we find to exist. If such coexistences and sequences as those of Biology and Sociology are not yet reduced to law, the presumption is not that they are irreducible to law, but that their laws elude our present means of analysis. Having long ago proved uniformity throughout all the lower classes of relations, and having been step by step proving uniformity throughout classes of relations successively higher and higher, if we have not yet succeeded with the highest classes, it may

be fairly concluded that our powers are at fault, rather than that the uniformity does not exist. And unless we make the absurd assumption that the process of generalization, now going on with unexampled rapidity, has reached its limit, and will suddenly cease, we must infer that ultimately mankind will discover a constant order of manifestation even in the most involved and obscure phenomena.

IV.

THE ORIGIN OF ANIMAL-WORSHIP.

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THE ORIGIN OF ANIMAL-WORSHIP.

Mr. McLennan's recent essays on the Worship of Animals and Plants have done much to elucidate a very obscure subject. By pursuing in this case, as before in another case, the truly scientific method of comparing the phenomena presented by existing uncivilized races with those which the early traditions of civilized races present, he has rendered both more comprehensible than they were before.

It seems to me, however, that Mr. McLennan gives but an indefinite answer to the essential question—How did the worship of animals and plants arise? Indeed, in his concluding paper, he expressly leaves this problem without a solution; saying that his "is not an hypothesis explanatory of the origin of *Totemism*, be it remembered, but an hypothesis explanatory of the animal and plant worship of the ancient nations." So that we have still to ask—Why have savage tribes so generally taken animals and plants and other things as their totems? What can have induced this tribe to ascribe special sacredness to one creature, and that tribe to another? And if to these questions the general reply is, that each tribe considers itself to be descended from the object of its reverence, then there presses for answer the further question—How came so strange a notion into existence? If this notion occurred

in one case only, we might set it down to some whim of thought or some illusive occurrence. But appearing as it does with multitudinous variations among so many uncivilized races in different parts of the world, and having left equally numerous traces in the superstitions of the extinct civilized races, we cannot assume any special or exceptional cause. Moreover, the general cause, whatever it may be, must be such as does not negative an aboriginal intelligence essentially like our own. After studying the grotesque beliefs of savages, we are apt to suppose that their reason is not as our reason. But this supposition is inadmissible. Given the amount of knowledge which primitive men possess, and given the imperfect verbal symbols used by them in speech and thought, and the conclusions they habitually reach will be those that are *relatively* the most rational. This must be our postulate; and, setting out with this postulate, we have to ask how primitive men came so generally, if not universally, to believe themselves the progeny of animals or plants or inanimate bodies. There is, I believe, a satisfactory answer.

The proposition with which Mr. McLennan sets out, that totem-worship preceded the worship of anthropomorphic gods, is one to which I can yield but a qualified assent. It is true in a sense, but not wholly true. If the words "gods" and "worship" carry with them their ordinary definite meanings, the statement is true; but if their meanings are widened so as to comprehend those earliest vague notions out of which the definite ideas of gods and worship are evolved, I think it is not true. The rudimentary form of all religion is the propitiation of dead ancestors, who are supposed to be still existing, and to be capable of working good or evil to their descendants. As a preparation for dealing hereafter with the principles of

sociology, I have, for some years past, directed much attention to the modes of thought current in the simpler human societies; and evidence of many kinds, furnished by all varieties of uncivilized men, has forced on me a conclusion harmonizing with that lately expressed in this Review by Prof. Huxley—namely, that the savage, conceiving a corpse to be deserted by the active personality who dwelt in it, conceives this active personality to be still existing, and that his feelings and ideas concerning it form the basis of his superstitions. Everywhere we find expressed or implied the belief that each person is double; and that when he dies, his other self, whether remaining near at hand or gone far away, may return, and continues capable of injuring his enemies and aiding his friends.[1]

[1] A critical reader may raise an objection. If animal-worship is to be rationally interpreted, how can the interpretation set out by assuming a belief in the spirits of dead ancestors—a belief which just as much requires explanation? Doubtless there is here a wide gap in the argument. I hope eventually to fill it up. Here, out of many experiences which conspire to generate this belief, I can but briefly indicate the leading ones: 1. It is not impossible that his shadow, following him everywhere, and moving as he moves, may have some small share in giving to the savage a vague idea of his duality. It needs but to watch a child's interest in the movements of its shadow, and to remember that at first a shadow cannot be interpreted as a negation of light, but is looked upon as an entity, to perceive that the savage may very possibly consider it as a specific something which forms part of him. 2. A much more decided suggestion of the same kind is likely to result from the reflection of his face and figure in water: imitating him as it does in his form, colors, motions, grimaces. When we remember that not unfrequently a savage objects to have his portrait taken, because he thinks whoever carries away a representation of him carries away some part of his being, will see how probable it is that he thinks his double in the water is a reality in some way belonging to him. 3. Echoes must greatly tend to confirm the idea of duality otherwise arrived at. Incapable as he is of understanding their natural origin, the primitive man necessarily ascribes them to living beings—beings who mock him and elude his search. 4. The suggestions resulting from these and other physical phenomena are, however, secondary in importance. The root of this belief in another self lies in the experience of dreams. The distinction so easily made by us between our life in dreams and our real life, is one which the savage recognizes in but a vague way; and he cannot express even that distinction which he perceives. When he awakes, and to those who have seen

But how out of the desire to propitiate this second personality of a deceased man (the words "ghost" or "spirit" are somewhat misleading, since the savage believes that the second personality reappears in a form equally tangible with the first) does there grow up the worship of

him lying quietly asleep, describes where he has been, and what he has done, his rude language fails to state the difference between seeing and dreaming that he saw, doing and dreaming that he did. From this inadequacy of his language it not only results that he cannot truly represent this difference to others, but also that he cannot truly represent it to himself. Hence, in the absence of an alternative interpretation, his belief, and that of those to whom he tells his adventures, is that his other self has been away and came back when he awoke. And this belief, which we find among various existing savage tribes, we equally find in the traditions of the early civilized races. 5. The conception of another self capable of going away and returning, receives what to the savage must seem conclusive verifications from the abnormal suspensions of consciousness, and derangements of consciousness, that occasionally occur in members of his tribe. One who has fainted, and cannot be immediately brought back to himself (note the significance of our own phrases "returning to himself," etc.) as a sleeper can, shows him a state in which the other self has been away for a time beyond recall. Still more is this prolonged absence of the other self shown him in cases of apoplexy, catalepsy, and other forms of suspended animation. Here for hours the other self persists in remaining away, and on returning refuses to say where he has been. Further verification is afforded by every epileptic subject, into whose body, during the absence of the other self, some enemy has entered; for how else does it happen that the other self on returning denies all knowledge of what his body has been doing? And this supposition that the body has been "possessed" by some other being, is confirmed by the phenomena of somnambulism and insanity. 6. What, then, is the interpretation inevitably put upon death? The other self has habitually returned after sleep, which simulates death. It has returned, too, after fainting, which simulates death much more. It has even returned after the rigid state of catalepsy, which simulates death very greatly. Will it not return also after this still more prolonged quiescence and rigidity? Clearly it is quite possible—quite probable even. The dead man's other self is gone away for a long time, but it still exists somewhere, far or near, and may at any moment come back to do all he said he would do. Hence the various burial-rites—the placing of weapons and valuables along with the body, the daily bringing of food to it, etc. I hope hereafter to show that, with such knowledge of the facts as he has, this interpretation is the most reasonable the savage can arrive at. Let me here, however, by way of showing how clearly the facts bear out this view, give one illustration out of many. "The ceremonies with which they [the Veddahs] invoke them [the shades of the dead] are few as they are simple. The most common

animals, plants, and inanimate objects? Very simply. Savages habitually distinguish individuals by names that are either directly suggestive of some personal trait or fact of personal history, or else express an observed community of character with some well-known object. Such a genesis of individual names, before surnames have arisen, is inevitable; and how easily it arises we shall see on remembering that it still goes on in its original form, even when no longer needful. I do not refer only to the significant fact that in some parts of England, as in the nail-making districts, nicknames are universal, and surnames scarcely recognized; but I refer to the general usage among both children and adults. The rude man is apt to be known as "a bear;" a sly fellow, as an "old fox;" a hypocrite, as "the crocodile." Names of plants, too, are used; as when the red-haired boy is called "carrots" by his school-fellows. Nor do we lack nicknames derived from inorganic objects and agents: instance that given by Mr. Carlyle to the elder Sterling—"Captain Whirlwind." Now, in the earliest savage state, this metaphorical nam-

is the following: An arrow is fixed upright in the ground, and the Veddah dances slowly round it, chanting this invocation, which is almost musical in its rhythm:

"Mà miya, mà miy, mà deyà,
Topang Koyichetti mittigan yandàh?"

"My departed one, my departed one, my God!
Where art thou wandering?"

"This invocation appears to be used on all occasions when the intervention of the guardian spirits is required in sickness, preparatory to hunting, etc. Sometimes in the latter case, a portion of the flesh of the game is promised as a votive offering, in the event of the chase being successful; and they believe that the spirits will appear to them in dreams and tell them where to hunt. Sometimes they cook food and place it in the dry bed of a river, or some other secluded spot, and then call on their deceased ancestors by name, 'Come and partake of this! Give us maintenance as you did when living! Come, wheresoever you may be, on a tree, on a rock, in the forest, come!' And dance round the food, half chanting half shouting the invocation."—*Bailey*, *Trans. Eth. Soc.*, London, N. S., ii., p. 301.

ing will in most cases commence afresh in each generation—must do so, indeed, until surnames of some kind have been established. I say in most cases, because there will occur exceptions in the cases of men who have distinguished themselves. If "the Wolf," proving famous in fight, becomes a terror to neighboring tribes, and a dominant man in his own, his sons, proud of their parentage, will not let fall the fact that they descended from the Wolf; nor will this fact be forgotten by the rest of the tribe who held "the Wolf" in awe, and see some reason to dread his sons. In proportion to the power and celebrity of the Wolf will this pride and this fear conspire to maintain among his grandchildren and great-grandchildren, as well as among those over whom they dominate, the remembrance of the fact that their ancestor was the Wolf. And if, as will occasionally happen, this dominant family becomes the root of a new tribe, the members of this tribe will become known to themselves and others as the Wolves.

We need not rest satisfied with the inference that this inheritance of nicknames *will* take place: there is proof that it *does* take place. As nicknaming after animals, plants, and other objects, still goes on among ourselves, so among ourselves does there go on the descent of nicknames. An instance has come under my own notice on an estate in the West Highlands, belonging to some friends with whom I frequently have the pleasure of spending a few weeks in the autumn. "Take a young Croshek," has more than once been the reply of my host to the inquiry, who should go with me when I was setting out salmon-fishing. The elder Croshek I knew well; and supposed that this name, borne by him and by all belonging to him, was the family surname. Some years passed before I learned that the real surname was Cameron; that the

father was called Croshek, after the name of his cottage, to distinguish him from other Camerons employed about the premises; and that his children had come to be similarly distinguished. Though here, as very generally in Scotland, the nickname was derived from the place of residence, yet had it been derived from an animal, the process would have been the same—inheritance of it would have occurred just as naturally. Not even for this small link in the argument, however, need we depend on inference: there is fact to bear us out. Mr. Bates, in his "Naturalist on the River Amazon" (2d ed., p. 376), describing three half-castes who accompanied him on a hunting trip, says: "Two of them were brothers—namely, João (John) and Zephyrino Jabutí; Jabutí, or tortoise, being a nickname which their father had earned for his slow gait, and which, as is usual in this country, had descended as the surname of the family." Let me add the statement made by Mr. Wallace respecting this same region, that "one of the tribes on the river Isánna is called 'Jurupari' (Devils). Another is called 'Ducks;' a third, 'Stars;' a fourth, 'Mandiocca.'" Putting these two statements together, can there be any doubt about the genesis of these tribal names? Let the tortoise become sufficiently distinguished (not necessarily by superiority—great inferiority may occasionally suffice) and the tradition of descent from him, preserved by his descendants themselves if he was superior, and by their contemptuous neighbors if he was inferior, may become a tribal name.[1]

[1] Since the foregoing pages were written, my attention has been drawn by Sir John Lubbock to a passage in the appendix to the second edition of "Prehistoric Times," in which he has indicated this derivation of tribal names. He says: "In endeavoring to account for the worship of animals, we must remember that names are very frequently taken from them. The children and followers of a man called the Bear or the Lion would make that a tribal name. Hence the animal itself would be first respected, at last worshipped." Of the genesis of this worship, however, Sir John Lubbock does not give any specific

"But this," it will be said, "does not amount to an explanation of animal-worship." True: a third factor remains to be specified. Given a belief in the still-existing other self of the deceased ancestor, who must be propitiated; given this survival of his metaphorical name among his grandchildren, great-grandchildren, etc.; and the further requisite is that the distinction between metaphor and reality shall be forgotten. Let the tradition of the ancestor fail to keep clearly in view the fact that he was a man called the Wolf—let him be habitually spoken of as the Wolf, just as when alive; and the natural mistake of taking the name literally will bring with it, firstly, a belief in descent from the actual wolf, and, secondly, a treatment of the wolf in a manner likely to propitiate him—a manner appropriate to one who may be the other self of the dead ancestor, or one of the kindred, and therefore a friend.

That a misunderstanding of this kind will naturally grow up, becomes obvious when we bear in mind the great indefiniteness of primitive language. As Prof. Max Müller says, respecting certain misinterpretations of an opposite kind: "These metaphors . . . . would become mere names handed down in the conversation of a family, understood perhaps by the grandfather, familiar to the father, but strange to the son, and misunderstood by the grandson." We have ample reason, then, for thinking that such misinterpretations are likely to occur. Nay, we may go further. We are justified in saying that they are certain to occur. For undeveloped languages contain no words capable of indicating the distinction to be kept in view. In the tongues of existing inferior races, only con-

explanation. Apparently he inclines to the belief, tacitly adopted also by Mr. McLennan, that animal-worship is derived from an original Fetichism, of which it is a more developed form. As will shortly be seen, I take a different view of its origin.

crete objects and acts are expressible. The Australians have a name for each kind of tree, but no name for tree irrespective of kind. And though some witnesses allege that their vocabulary is not absolutely destitute of generic names, its extreme poverty in such is unquestionable. Similarly with the Tasmanians. Dr. Milligan says they "had acquired very limited powers of abstraction or generalization. They possessed no words representing abstract ideas; for each variety of gum-tree and wattle-tree, etc., etc., they had a name, but they had no equivalent for the expression, 'a tree;' neither could they express abstract qualities, such as hard, soft, warm, cold, long, short, round, etc.; for 'hard,' they would say 'like a stone,' for 'tall,' they would say 'long legs,' etc., and for 'round,' they said 'like a ball,' 'like the moon,' and so on, usually suiting the action to the word, and confirming, by some sign, the meaning to be understood."[1] Now, even making allowance for over-statement here (which seems needful, since the word "long," said to be inexpressible in the abstract, subsequently occurs as qualifying a concrete in the expression, "long legs"), it is sufficiently manifest that so imperfect a language must fail to convey the idea of a name, as something separate from a thing; and that still less can it be capable of indicating the act of naming. Familiar use of such partially abstract words as are applicable to all objects of a class, is needful before there can be reached the conception of a name—a word symbolizing the symbolic character of other words; and the conception of a name, with its answering abstract term, must be long current before the verb to name can arise. Hence, among tribes with speech so rude, it will be impossible to transmit the tradition of an ancestor named the Wolf, as distinguished from the actual wolf. The children and grand-

[1] Proc. Royal Soc. Tasmania, iii., p. 280.

children who saw him will not be led into error; but in later generations, descent from the Wolf will inevitably come to mean descent from the animal known by that name. And the ideas and sentiments which, as above shown, naturally grow up around the belief that the dead parents and grandparents are still alive, and ready, if propitiated, to befriend their descendants, will be extended to the wolf species.

Before passing to other developments of this general view, let me point out how not simply animal-worship is thus accounted for, but also the conception, so variously illustrated in ancient legends, that animals are capable of displaying human powers of speech and thought and action. Mythologies are full of stories of beasts and birds and fishes that have played intelligent parts in human affairs—creatures that have befriended particular persons by giving them information, by guiding them, by yielding them help; or else that have deceived them, verbally or otherwise. Evidently all these traditions, as well as those about abductions of women by animals and fostering of children by them, fall naturally into their places as results of the habitual misinterpretation I have described.

The probability of the hypothesis will appear still greater when we observe how readily it applies to the worship of other orders of objects. Belief in actual descent from an animal, strange as we may think it, is one by no means incongruous with the unanalyzed experiences of the savage; for there come under his notice many metamorphoses, vegetal and animal, which are apparently of like character. But how could he possibly arrive at so grotesque a conception as that the progenitor of his tribe was the sun, or the moon, or a particular star? No observation of surrounding phenomena affords the slightest

suggestion of any such possibility. But by the inheritance of nicknames that are eventually mistaken for the names of the objects from which they were derived, the belief readily arises—is sure to arise. That the names of heavenly bodies will furnish metaphorical names to the uncivilized, is manifest. Do we not ourselves call a distinguished singer or actor a star? And have we not in poems numerous comparisons of men and women to the sun and moon; as in "Love's Labour's Lost," where the princess is called "a gracious moon," and as in "Henry VIII.," where we read—"Those suns of glory, those two lights of men?" Clearly, primitive men will be not unlikely thus to speak of the chief hero of a successful battle. When we remember how the arrival of a triumphant warrior must affect the feelings of his tribe, dissipating clouds of anxiety and irradiating all faces with joy, we shall see that the comparison of him to the sun is extremely natural; and in early speech this comparison can be made only by calling him the sun. As before, then, it will happen that, through a confounding of the metaphorical name with the actual name, his progeny, after a few generations, will be regarded by themselves and others as descendants of the sun. And, as a consequence, partly of actual inheritance of the ancestral character, and partly of maintenance of the traditions respecting the ancestor's achievements, it will also naturally happen that the solar race will be considered a superior race, as we find it habitually is.

The origin of other totems, equally strange if not even stranger, is similarly accounted for, though otherwise unaccountable. One of the New-Zealand chiefs claimed as his progenitor the neighboring great mountain, Tongariro. This seemingly-whimsical belief becomes intelligible when we observe how easily it may have arisen from a nickname. Do we not ourselves sometimes speak figuratively

of a tall, fat man as a mountain of flesh? And, among a people prone to speak in still more concrete terms, would it not happen that a chief, remarkable for his great bulk, would be nicknamed after the highest mountain within sight, because he towered above other men as this did above surrounding hills? Such an occurrence is not simply possible, but probable. And, if so, the confusion of metaphor with fact would originate this surprising genealogy. A notion perhaps yet more grotesque, thus receives a satisfactory interpretation. What could have put it into the imagination of any one that he was descended from the dawn? Given the extremest credulity, joined with the wildest fancy, it would still seem requisite that the ancestor should be conceived as an entity; and the dawn is entirely without that definiteness and comparative constancy which enter into the conception of an entity. But when we remember that "the Dawn" is a natural complimentary name for a beautiful girl opening into womanhood, the genesis of the idea becomes, on the above hypothesis, quite obvious.

Another indirect verification is that we thus get a clear conception of Fetichism in general. Under the fetichistic mode of thought, surrounding objects and agents are regarded as having powers more or less definitely personal in their natures. And the current interpretation is, that human intelligence, in its early stages, is obliged to conceive of their powers under this form. I have myself hitherto accepted this interpretation; though always with a sense of dissatisfaction. This dissatisfaction was, I think, well grounded. The theory is scarcely a theory properly so called; but rather, a restatement in other words. Uncivilized men *do* habitually form anthropomorphic conceptions of surrounding things; and this ob-

served general fact is transformed into the theory that at first they *must* so conceive them—a theory for which the psychological justification attempted, seems to me inadequate. From our present stand-point, it becomes manifest that Fetichism is not primary but secondary. What has been said above almost of itself shows this. Let us, however, follow out the steps of its genesis. Respecting the Tasmanians, Dr. Milligan says: "The names of men and women were taken from natural objects and occurrences around, as, for instance, a kangaroo, a gum-tree, snow, hail, thunder, the wind, flowers in blossom, etc." Surrounding objects, then, giving origin to names of persons, and being, in the way shown, eventually mistaken for the actual progenitors of those who descend from persons nicknamed after them, it results that these surrounding objects come to be regarded as in some manner possessed of personalities like the human. He whose family tradition is that his ancestor was "the Crab," will conceive the crab as having a disguised inner power like his own; and alleged descent from "the palm-tree" will entail belief in some kind of consciousness dwelling in the palm-tree. Hence, in proportion as the animals, plants, and inanimate objects or agents that originate names of persons, become numerous (which they will do in proportion as a tribe becomes large and the number of persons to be distinguished from one another increases), multitudinous things around will acquire imaginary personalities. And so it will happen that, as Mr. McLennan says of the Feejeeans: "Vegetables and stones, nay, even tools and weapons, pots and canoes, have souls that are immortal, and that, like the souls of men, pass on at last to Mbulu, the abode of departed spirits." Setting out, then, with a belief in the still-living other self of the dead ancestor, the alleged general cause of misapprehension affords us an

intelligible origin of the fetichistic conception; and we are enabled to see how it tends to become a general, if not a universal, conception.

Other apparently inexplicable phenomena are at the same time divested of their strangeness. I refer to the beliefs in, and worship of, compound monsters—impossible hybrid animals, and forms that are half human, half brutal. The theory of a primordial Fetichism, supposing it otherwise adequate, yields no feasible solution of these. Grant the alleged original tendency to think of all natural agencies as in some way personal. Grant, too, that hence may arise a worship of animals, plants, and even inanimate bodies. Still the obvious implication is that the worship so derived will be limited to things that are, or have been, perceived. Why should this mode of thought lead the savage to imagine a combination of bird and mammal; and not only to imagine it, but worship it as a god? If even we admit that some illusion may have suggested the belief in a creature half man, half fish, we cannot thus explain the prevalence among Eastern races of idols representing bird-headed men, men having their legs replaced by the legs of a cock, and men with the heads of elephants.

Carrying with us the inferences above drawn, however, it is a manifest corollary that ideas and practices of these kinds will arise. When tradition preserves both lines of ancestry—when a chief, nicknamed the Wolf, carries away from an adjacent tribe a wife who is remembered either under the animal name of her tribe, or as a woman; it will happen that if a son distinguishes himself, the remembrance of him among his descendants will be that he was born of a wolf and some other animal, or of a wolf and a woman. Misinterpretation, arising in the way described from defects of language, will entail belief

in a creature uniting the attributes of the two; and if the tribe grows into a society, representations of such a creature will become objects of worship. One of the cases cited by Mr. McLennan may here be repeated in illustration. "The story of the origin of the Dikokamenni Kirgheez," they say, "from a red greyhound and a certain queen with her forty handmaidens, is of ancient date." Now, if "the red greyhound" was the nickname of a man extremely swift of foot (celebrated runners have been similarly nicknamed among ourselves), a story of this kind would naturally arise; and if the metaphorical name was mistaken for the actual name, there might result, as the idol of the race, a compound form appropriate to the story. We need not be surprised, then, at finding among the Egyptians the goddess Pasht represented as a woman with a lion's head, and the god Month as a man with the head of a hawk. The Babylonian gods—one having the form of a man with an eagle's tail, and another uniting a human bust to a fish's body—no longer appear such unaccountable conceptions. We get feasible explanations, too, of sculptures representing sphinxes, winged human-headed bulls, etc.; as well as of the stories about centaurs, satyrs, and the rest.

Ancient myths in general thus acquire meanings considerably different from those ascribed to them by comparative mythologists. Though these last may be in part correct, yet if the foregoing argument is valid, they can scarcely be correct in their main outlines. Indeed, if we read the facts the other way upward, regarding as secondary or additional the elements that are said to be primary, while we regard as primary certain elements which are considered as accretions of later times, we shall, I think, be nearer the truth.

The current theory of the myth is that it has grown out of the habit of symbolizing natural agents and processes, in terms of human personalities and actions. Now, it may in the first place be remarked that, though symbolization of this kind is common enough among civilized races, it is not common among races that are the most uncivilized. By existing savages, surrounding objects, motions, and changes, are habitually used to convey ideas respecting human transactions. It is by no means so much the habit to express by the doings of men the course of natural phenomena. It needs but to read the speech of an Indian chief to see that just as primitive men name one another metaphorically after surrounding objects, so do they metaphorically describe one another's doings as though they were the doings of natural objects. But assuming a contrary habit of thought to be the dominant one, ancient myths are explained as results of the primitive tendency to symbolize inanimate things and their changes, by human beings and their doings.

A kindred difficulty must be added. The change of verbal meaning from which the myth is said to arise, is a change opposite in kind to that which prevails in the earlier stages of linguistic development. It implies a derivation of the concrete from the abstract; whereas at first abstracts are derived only from concretes: the concreting of abstracts being a subsequent process. In the words of Prof. Max Müller, there are "dialects spoken at the present day which have no abstract nouns, and the more we go back in the history of languages, the smaller we find the number of these useful expressions" ("Chips," vol. ii., p. 54); or, as he says more recently: "Ancient words and ancient thoughts, for both go together, have not yet arrived at that stage of abstraction in which, for instance, active powers, whether natural or supernatural,

can be represented in any but a personal and more or less human form." (*Fraser's Magazine*, April, 1870.) Here the concrete is represented as original, and the abstract as derivative. Immediately afterward, however, Prof. Max Müller, having given as examples of abstract nouns, "day and night, spring and winter, dawn and twilight, storm and thunder," goes on to argue that, "as long as people thought in language, it was simply impossible to speak of morning or evening, of spring and winter, without giving to these conceptions something of an individual, active, sexual, and at last personal character." ("Chips," etc., vol. ii., p. 55.) Here the concrete is derived from the abstract—the personal conception is represented as coming *after* the impersonal conception; and through such transformation of the impersonal into the personal, Prof. Max Müller considers ancient myths to have arisen. How are these propositions reconcilable? One of two things must be said: If originally there were none of these abstract nouns, then the earliest statements respecting the daily course of Nature were made in concrete terms—the personal elements of the myth were the primitive elements, and the impersonal expressions which are their equivalents came later. If this is not admitted, then it must be held that, until after there arose these abstract nouns, there were no current statements at all respecting these most conspicuous objects and changes which the heavens and the earth present; and that the abstract nouns having been somehow formed, and rightly formed, and used without personal meanings, afterward became personalized—a process the reverse of that which characterizes early linguistic progress.

No such contradictions occur if we interpret myths after the manner that has been indicated. Nay, besides escaping contradictions, we meet with unexpected solu-

tions. The moment we try it, the key unlocks for us with ease what seems a quite inexplicable fact, which the current hypothesis takes as one of its postulates. Speaking of such words as sky and earth, dew and rain, rivers and mountains, as well as of the abstract nouns above named, Prof. Max Müller says: "Now, in ancient languages every one of these words had necessarily a termination expressive of gender, and this naturally produced in the mind the corresponding idea of sex, so that these names received not only an individual but a sexual character. There was no substantive which was not either masculine or feminine; neuters being of later growth, and distinguishable chiefly in the nominative." ("Chips," etc., vol. ii., p. 55.) And this alleged necessity for a masculine or feminine implication is assigned as a part of the reason why these abstract nouns and collective nouns became personalized. But should not a true theory of these first steps in the evolution of thought and language show us how it happened that men acquired the seemingly-strange habit of so framing their words for sky, earth, dew, rain, etc., as to make them indicative of sex? Or, at any rate, must it not be admitted that an interpretation which, instead of assuming this habit to be "necessary," shows us how it results, thereby acquires an additional claim to acceptance? The interpretation I have indicated does this. If men and women are habitually nicknamed, and if defects of language lead their descendants to regard themselves as descendants of the things from which the names were taken, then masculine or feminine genders will be ascribed to these things according as the ancestors named after them were men or women. If a beautiful maiden known metaphorically as "the Dawn," afterward becomes the mother of some distinguished chief called "the North Wind," it will result that when, in course of

time, the two have been mistaken for the actual dawn and the actual north wind, these will, by implication, be respectively considered as male and female.

Looking, now, at the ancient myths in general, their seemingly most inexplicable trait is the habitual combination of alleged human ancestry and adventures, with the possession of personalities otherwise figuring in the heavens and on the earth, with totally non-human attributes. This enormous incongruity, not the exception but the rule, the current theory fails to explain. Suppose it to be granted that the great terrestrial and celestial objects and agents naturally become personalized; it does not follow that each of them shall have a specific human biography. To say of some star that he was the son of this king or that hero, was born in a particular place, and when grown up carried off the wife of a neighboring chief, is a gratuitous multiplication of incongruities already sufficiently great; and is not accounted for by the alleged necessary personalization of abstract and collective nouns. As looked at from our present stand-point, however, such traditions become quite natural—nay, it is clear that they will necessarily arise. When a nickname has become a tribal name, it thereby ceases to be individually distinctive; and, as already said, the process of nicknaming inevitably continues. It commences afresh with each child; and the nickname of each child is both an individual name and a potential tribal name, which may become an actual tribal name if the individual is sufficiently celebrated. Usually, then, there is a double system of distinguishing the individual; under one of which he is known by his ancestral name, and under the other of which he is known by a name suggestive of something peculiar to himself: just as we have seen happens among the Scotch clans. Consider, now, what will result when language

has reached a stage of development such that it can convey the notion of naming, and is able, therefore, to preserve traditions of human ancestry: the preservation of such traditions being furthered by those corruptions of tribal names which render them no longer suggestive of the things they were derived from. It will result that the individual will be known both as the son of such and such a man by a mother whose name was so-and-so, and also as the Crab, or the Bear, or the Whirlwind—supposing one of these to be his nickname. Such joint use of nicknames and proper names occurs in every school. Now, clearly, in advancing from the early state in which ancestors become identified with the objects they are nicknamed after, to the state in which there are proper names that have lost their metaphorical meanings, there must be passed through a state in which proper names, partially settled only, may or may not be preserved, and in which the new nicknames are still liable to be mistaken for actual names. Under such conditions there will arise (especially in the case of a distinguished man) this seemingly-impossible combination of human parentage with the possession of the non-human, or superhuman, attributes of the thing which gave the nickname. Another anomaly simultaneously disappears. The warrior may have, and often will have, a variety of complimentary nicknames—"the powerful one," "the destroyer," etc. Supposing his leading nickname has been the Sun, then when he comes to be identified by tradition with the sun, it will happen that the sun will acquire his alternative descriptive titles—the swift one, the lion, the wolf—titles not obviously appropriate to the sun, but quite appropriate to the warrior. Then there comes, too, an explanation of the remaining trait of such myths. When this identification of conspicuous persons, male and female, with con-

spicuous natural agents, has become settled, there will in due course arise interpretations of the actions of these agents in anthropomorphic terms. Suppose, for instance, that Endymion and Selene, metaphorically named, the one after the setting sun, the other after the moon, have had their human individualities merged in those of the sun and moon, through misinterpretation of metaphors; what will happen? The legend of their loves having to be reconciled with their celestial appearances and motions, these will be spoken of as results of feeling and will; so that when the sun is going down in the west, while the moon in mid-heaven is following him, the fact will be expressed by saying: "Selene loves and watches Endymion." Thus we obtain a consistent explanation of the myth without distorting it; and without assuming that it contains gratuitous fictions. We are enabled to accept the biographical part of it, if not as literal fact, still as having had fact for its root. We are helped to see how, by an inevitable misinterpretation, there grew out of a more or less true tradition, this strange identification of its personages, with objects and powers totally non-human in their aspects. And then we are shown how, from the attempt to reconcile in thought these contradictory elements of the myth, there arose the habit of ascribing the actions of these non-human things to human motives.

One further verification may be drawn from facts which are obstacles to the converse hypothesis. These objects and powers, celestial and terrestrial, which force themselves most on men's attention, have some of them several proper names, identified with those of different individuals, born at different places, and having different sets of adventures. Thus we have the sun variously known as Apollo, Endymion, Helios, Tithonos, etc.—personages having irreconcilable genealogies. Such anoma-

lies Prof. Max Müller apparently ascribes to the untrustworthiness of traditions, which are "careless about contradictions, or ready to solve them sometimes by the most atrocious expedients." ("Chips," etc., vol. ii., p. 84.) But if the evolution of the myth has been that above indicated, there exist no anomalies to be got rid of: these diverse genealogies become parts of the evidence. For we have abundant proof that the same objects furnish metaphorical names of men in different tribes. There are Duck tribes in Australia, in South America, in North America. The eagle is still a totem among the North Americans, as Mr. McLennan shows reason to conclude that it was among the Egyptians, among the Jews, and among the Romans. Obviously, for reasons that have been assigned, it naturally happened in the early stages of the ancient races, that complimentary comparisons of their heroes to the sun were frequently made. What resulted? The sun having furnished names for sundry chiefs and early founders of tribes, and local traditions having severally identified them with the sun, these tribes, when they grew, spread, conquered, or came otherwise into partial union, originated a combined mythology, which necessarily contained conflicting stories about the sun-god, as about its other leading personages. If the North-American tribes, among several of which there are traditions of a sun-god, had developed a combined civilization, there would similarly have arisen among them a mythology which ascribed to the sun several different proper names and genealogies.

Let me briefly set down the leading characters of this hypothesis which give it probability.

True interpretations of all the natural processes, organic and inorganic, that have gone on in past times, habitually trace them to causes still in action. It is thus

in Geology; it is thus in Biology; it is thus in Philology. Here we find this characteristic repeated. Nicknaming, the inheritance of nicknames, and, to some extent, the misinterpretation of nicknames, go among us still; and were surnames absent, language imperfect, and knowledge as rudimentary as of old, it is tolerably manifest that results would arise like those we have contemplated.

A further characteristic of a true cause is that it accounts not only for the particular group of phenomena to be interpreted, but also for other groups. The cause here alleged does this. It equally well explains the worship of animals, of plants, of mountains, of winds, of celestial bodies, and even of appearances too vague to be considered entities. It gives us an intelligible genesis of fetichistic conceptions in general. It furnishes us with a reason for the practice, otherwise so unaccountable, of moulding the words applied to inanimate objects in such ways as to imply masculine and feminine genders. It shows us how there naturally arose the worship of compound animals, and of monsters half man half brute. And it shows us why the worship of purely anthropomorphic deities came later, when language had so far developed that it could preserve in tradition the distinction between proper names and nicknames.

A further verification of this view is, that it conforms to the general law of evolution: showing us how, out of one simple, vague, aboriginal form of belief, there have arisen, by continuous differentiations, the many heterogeneous forms of belief which have existed and do exist. The desire to propitiate the other self of the dead ancestor, displayed among savage tribes, dominantly manifested by the early historic races, by the Peruvians and Mexicans, by the Chinese at the present time, and to a considerable degree by ourselves (for what else is the wish to do

that which a lately-deceased parent was known to have desired?), has been the universal first form of religious belief; and from it have grown up the many divergent beliefs that have been referred to.

Let me add, as a further reason for adopting this view, that it immensely diminishes the apparently-great contrast between early modes of thought and our own mode of thought. Doubtless the aboriginal man differs considerably from us, both in intellect and feeling. But such an interpretation of the facts as helps us to bridge over the gap, derives additional likelihood from doing this. The hypothesis I have sketched out enables us to see that primitive ideas are not so gratuitously absurd as we suppose, and also enables us to rehabilitate the ancient myth with far less distortion than at first sight appears possible.

These views I hope to develop in the first part of "The Principles of Sociology." The large mass of evidence which I shall be able to give in support of the hypothesis, joined with the solutions it will be shown to yield of many minor problems which I have passed over, will, I think, then give to it a still greater probability than it seems now to have.

V.

SPECIALIZED ADMINISTRATION.

[FROM THE FORTNIGHTLY REVIEW, DECEMBER, 1871.]

SPECIALIZED ADMINISTRATION.

It is contrary to common-sense that fish should be more difficult to get at the sea-side than in London; but it is true, nevertheless. No less contrary to common-sense seems the truth that though, in the West Highlands, oxen are to be seen everywhere, no beef can be had without sending two or three hundred miles to Glasgow for it. Rulers who, guided by common-sense, tried to suppress certain opinions by forbidding the books containing them, never dreamed that their interdicts would cause the diffusion of these opinions; and rulers who, guided by common-sense, forbade excessive rates of interest, never dreamed that they were thereby making the terms harder for borrowers than before. When printing replaced copying, any one who had prophesied that the number of persons engaged in the manufacture of books would immensely increase, as a consequence, would have been thought wholly devoid of common-sense. And equally devoid of common-sense would have been thought any one who, when railways were replacing coaches, said that the number of horses employed in bringing passengers and goods to and from railways, would be greater than the number directly replaced by the railways. Such cases might be multiplied indefinitely. Whoso remembers that, among quite simple phenomena, causes produce effects

which are often utterly at variance with anticipation, will see how habitually this must happen among complex phenomena. That a balloon is made to rise by the same force which makes a stone fall; that the melting of ice may be greatly retarded by wrapping the ice in a blanket; that the simplest way of setting potassium on fire is to throw it into the water; are truths which those who know only the outside aspects of things would regard as manifest falsehoods. And, if, when the factors are few and simple, the results may be so absolutely opposed to seeming probability, much more will they be often thus opposed when the factors are many and involved. The saying of the French respecting political events, that "it is always the unexpected which happens"—a saying which they have been abundantly reillustrating of late—is one which legislators, and those who urge on schemes of legislation, should have ever in mind. Let us pause a moment to contemplate a seemingly-impossible set of results which social forces have wrought out.

Up to quite recent days, Language was held to be of supernatural origin. That this elaborate apparatus of symbols, so marvellously adapted for the conveyance of thought from mind to mind, was a miraculous gift, seemed unquestionable. No possible alternative way could be thought of by which there had come into existence these multitudinous assemblages of words of various orders, genera, and species, moulded into fitness for articulating with one another, and capable of being united from moment to moment into ever-new combinations, that represent with precision each idea as it arises. The supposition that, in the slow progress of things, Language grew out of the continuous use of signs—at first mainly mimetic, afterward partly mimetic, partly vocal, and at length almost wholly vocal—was an hypothesis never even con-

ceived by men in early stages of civilization; and when the hypothesis was at length conceived, it was thought too monstrous an absurdity to be even entertained. Yet this monstrous absurdity proves to be true. Already the evolution of Language has been traced back far enough to show that all its particular words, and all its leading traits of structure, have had a natural genesis; and day by day investigation makes it more manifest that its genesis has been natural from the beginning. Not only has it been natural from the beginning, but it has been spontaneous. No language is a cunningly-devised scheme of a ruler or body of legislators. There was no council of savages to invent the parts of speech, and decide on what principles they should be used. Nay, more. Going on without any authority or appointed regulation, this natural process went on without any man observing that it was going on. Solely under pressure of the need for communicating their ideas and feelings—solely in pursuit of their personal interests—men little by little developed speech in absolute unconsciousness that they were doing any thing more than pursuing their personal interests. Even now the unconsciousness continues. Take the whole population of the globe, and there is probably not above one in a million who knows that in his daily talk he is carrying on the process by which Language has been evolved.

I commence thus by way of giving the key-note to the argument which follows. My general purpose, in dwelling a moment on this illustration, has been that of showing how utterly beyond the conceptions of common-sense, literally so called, and even beyond the conceptions of cultivated common-sense, are the workings-out of sociological processes—how these workings-out are such that even those who have carried to the uttermost "the scien-

tific use of the imagination," would never have anticipated them. And my more special purpose has been that of showing how marvellous are the results indirectly and unintentionally achieved by the coöperation of men who are severally pursuing their private ends. Let me pass now to the particular topic to be here dealt with.

I have greatly regretted to see Prof. Huxley strengthening, by his deservedly high authority, a school of politicians which can scarcely be held to need strengthening —its opponents being so few. I regret it the more because, thus far, men prepared for the study of Sociology by previous studies of Biology and Psychology, have scarcely expressed any opinions on the question at issue; and that Prof. Huxley, who by both general and special culture is so eminently fitted to judge, should have come to the conclusions set forth in the last number of the *Fortnightly Review*, will be discouraging to the small number who have reached opposite conclusions. Greatly regretting however, though I do, this avowed antagonism of Prof. Huxley to a general political doctrine with which I am identified, I do not propose to make any reply to his arguments at large: being deterred partly by reluctance to dwell on points of difference with one whom I so greatly admire, and partly by the consciousness that what I should say would be mainly a repetition of what I have explicitly or implicitly said elsewhere. But with one point raised I feel obliged to deal. Prof. Huxley tacitly puts to me a question. By so doing he leaves me to choose between two alternatives, neither of which is agreeable to me. I must either, by leaving it unanswered, accept the implication that it is unanswerable, and the doctrine I hold untenable; or else I must give it an adequate answer. Little as I like it, I see that the latter of these alternatives

is that which, on public as well as on personal grounds, I must accept.

Had I been allowed to elaborate more fully the Review-article from which Prof. Huxley quotes, this question would possibly not have been raised. That article closes with the following words: "We had hoped to say something respecting the different types of social organization, and something also on social metamorphoses; but we have reached our assigned limits." These further developments of the conception—developments to be hereafter set forth in the "Principles of Sociology"—I must here sketch in outline before my answer can be made intelligible. In sketching them, I must say much that would be needless were my answer addressed to Prof. Huxley only. Bare allusions to general phenomena of organization, with which he is immeasurably more familiar than I am, would suffice. But, as the sufficiency of my answer has to be judged by the general reader, the general reader must be supplied with the requisite data—my presentation of them being under correction from Prof. Huxley if it is inaccurate.

The primary differentiation in organic structures, manifested alike in the history of each organism and in the history of the organic world as a whole, is the differentiation between outer and inner parts—the parts which hold direct converse with the environment and the parts which do not hold direct converse with the environment. We see this alike in those smallest and lowest forms improperly, though suggestively, sometimes called unicellular, and also in the next higher division of creatures which, with considerable reason, are regarded as aggregations of the lower. In these creatures the body is divisible into endoderm and ectoderm, differing very little in their characters,

but serving the one to form the digestive sac, and the other to form the outer wall of the body. As Prof. Huxley describes them in his "Oceanic Hydrozoa," these layers represent respectively the organs of nutrition and the organs of external relation—generally, though not universally, for there are exceptions, especially among parasites. In the embryos of higher types, these two layers severally become double by the splitting of a layer formed between them; and from the outer double layer is developed the body-wall with its limbs, nervous system, senses, muscles, etc.; while from the inner double layer there arise the alimentary canal and its appendages, together with the heart and lungs. Though in such higher types these two systems of organs, which respectively absorb nutriment and expend nutriment, become so far connected by ramifying blood-vessels and nerves that this division cannot be sharply made, still the broad contrast remains. At the very outset, then, there arises this separation, which implies at once a coöperation and an antagonism—a coöperation, because, while the outer organs secure for the inner organs the crude food, the inner organs elaborate and supply to the outer organs the prepared materials by which they are enable to do their work; and an antagonism, because each set of organs, living and growing at the cost of these prepared materials, cannot appropriate any portion of the total supply without diminishing by so much the supply available for the other. This general coöperation and general antagonism becomes complicated with special coöperations and special antagonisms, as fast as these two great systems of organs develop. The originally simple alimentary canal, differentiating into many parts, becomes a congeries of structures which, by coöperation, fulfil better their general function, but between which there nevertheless arise antagonisms; since each has to make good its

waste and to get matter for growth, at the cost of the general supply of nutriment available for them all. Similarly, as fast as the outer system develops into special senses and limbs, there arise among these, also, secondary coöperations and secondary antagonisms. By their variously-combined actions, food is obtained more effectually; and yet the activity of each set of muscles, or each directive nervous structure, entails a draft upon the stock of prepared nutriment which the outer organs receive, and is by so much at the cost of the rest. Thus the method of organization, both in general and in detail, is a simultaneous coöperation and opposition. All the organs unite in subserving the interests of the organism they form; and yet they have all their special interests, and compete with one another for blood.

A form of government, or control, or coördination, develops as fast as these systems of organs develop. Eventually this becomes double. A general distinction arises between the two controlling systems belonging to the two great systems of organs. Whether the inner controlling system is or is not originally derived from the outer, matters not to the argument—when developed it is in great measure independent.* And if we contemplate their re-

* Here, and throughout the discussion, I refer to these controlling systems only as they exist in the *Vertebrata*, because their relations are far better known in this great division of the animal kingdom—not because like relations do not exist elsewhere. Indeed, in the great sub-kingdom *Annulosa*, these controlling systems have relations that are extremely significant to us here. For while an inferior annulose animal has only a single set of nervous structures, a superior annulose animal (as a moth) has a set of nervous structures presiding over the viscera, as well as a more conspicuous set presiding over the organs of external relation. And this contrast is analogous to one of the contrasts between undeveloped and developed societies; for, while among the uncivilized and incipiently civilized there is but a single set of directive agencies, there are among the fully civilized, as we shall presently see, two sets of directive agencies, for the outer and inner structures respectively.

spective sets of functions, we shall perceive the origin of this distinction. That the outer organs may coöperate effectively for the purposes of catching prey, escaping danger, etc., it is needful that they should be under a government capable of directing their combined actions, now in this way and now in that, according as outer circumstances vary. From instant to instant there must be quick adjustments to occasions that are more or less new; and hence there requires a complex and centralized nervous apparatus, to which all these organs are promptly and completely obedient. The government needful for the inner system of organs is a different and much simpler one. When the food obtained by the outer organs has been put into the stomach, the coöperation required of the viscera, though it varies somewhat as the quantity or kind of food varies, has nevertheless a general uniformity; and it is required to go on in much the same way whatever the outer circumstances may be. In each case the food has to be reduced to a pulp, supplied with various solvent secretions, propelled onward, and its nutritive part taken up by absorbent surfaces. That these processes may be effective, the organs which carry them on must be supplied with fit blood; and to this end the heart and the lungs have to act with greater vigor. This visceral coöperation, carried on with this comparative uniformity, is regulated by a nervous system which is to a large extent independent of that higher and more complex nervous system controlling the external organs. The act of swallowing is, indeed, mainly effected by the higher nervous system; but, being swallowed, the food affects by its presence the local nerves, through them the local ganglia, and indirectly, through nervous connections with other ganglia, excites the rest of the viscera into coöperative activity. It is true that the functions of the sympathetic or ganglionic nervous system,

or "nervous system of organic life," as it is otherwise called, are imperfectly understood. But, since we know positively that some of its plexuses, as the cardiac, are centres of local stimulation and coördination, which can act independently, though they are influenced by higher centres, it is fairly to be inferred that the other and still larger plexuses, distributed among the viscera, are also such local and largely independent centres; especially as the nerves they send into the viscera, to join the many subordinate ganglia distributed through them, greatly exceed in quantity the cerebro-spinal fibres accompanying them. Indeed, to suppose otherwise is to leave unanswered the question, What are their functions? as well as the question, How are these unconscious visceral coördinations effected? There remains only to observe the kind of coöperation which exists between the two nervous systems. This is both a general and a special coöperation. The general coöperation is that by which either system of organs is enabled to stimulate the other to action. The alimentary canal yields through certain nervous connections the sensation of hunger to the higher nervous system; and so prompts efforts for procuring food. Conversely, the activity of the nervo-muscular system, or, at least, its normal activity, sends inward to the cardiac and other plexuses a gush of stimulus which excites the viscera to action. The special coöperation is one by which it would seem that each system puts an indirect restraint on the other. Fibres from the sympathetic accompany every artery throughout the organs of external relation, and exercise on the artery a constrictive action; and the converse is done by certain of the cerebro-spinal fibres which ramify with the sympathetic throughout the viscera: through the vagus and other nerves, an inhibitory influence is exercised on the heart, intestines, pancreas, etc. Leaving

doubtful details, however, the fact which concerns us here is sufficiently manifest. There are, for these two systems of organs, two nervous systems, in great measure independent; and, if it is true that the higher system influences the lower, it is no less true that the lower very powerfully influences the higher. The restrictive action of the sympathetic upon the circulation, throughout the nervo-muscular system, is unquestionable; and it is possibly through this that, when the viscera have much work to do, the nervo-muscular system is incapacitated in so marked a manner.*

The one further fact here concerning us is the contrast presented in different kinds of animals, between the degrees of development of these two great systems of organs that carry on respectively the outer functions and the inner functions. There are active creatures in which the locomotive organs, the organs of sense, together with the nervous apparatus which combines their actions, bear a large ratio to the organs of alimentation and their appendages; while there are inactive creatures in which these

* To meet the probable objection that the experiments of Bernard, Ludwig, and others, show that in the case of certain glands the nerves of the cerebro-spinal system are those which set up the secreting process, I would remark that in these cases, and in many others where the relative functions of the cerebro-spinal nerves and the sympathetic nerves have been studied, the organs have been those in which *sensation* is either the stimulus to activity or its accompaniment; and that from these cases no conclusion can be drawn applying to the cases of those viscera which normally perform their functions without sensation. Perhaps it may even be that the functions of those sympathetic fibres which accompany the arteries of the outer organs are simply ancillary to those of the central parts of the sympathetic system, which stimulate and regulate the viscera—ancillary in this sense, that they check the diffusion of blood in external organs when it is wanted in internal organs: cerebro-spinal inhibition (except in its action on the heart) working the opposite way. And possibly this is the instrumentality for carrying on that competition for nutriment which, as we saw, arises at the very outset between these two great systems of organs.

organs of external relation bear a very small ratio to the organs of alimentation. And a remarkable fact, here especially instructive to us, is that very frequently there occurs a metamorphosis, which has for its leading trait a great change in the ratio of these two systems—a metamorphosis which accompanies a great change in the mode of life. The most familiar metamorphosis is variously illustrated among insects. During the early or larval stage of a butterfly, the organs of alimentation are largely developed, while the organs of external relation are but little developed; and then, during a period of quiescence the organs of external relation undergo an immense develment, making possible the creature's active and varied adjustments to the surrounding world, while the alimentary system becomes relatively small. On the other hand, among the lower invertebrate animals there is a very common metamorphosis of an opposite kind. When young, the creature, with scarcely any alimentary system, but supplied with limbs and sense organs, swims about actively. Presently it settles in a *habitat* where food is to be obtained without moving about, loses in great part its organs of external relation, develops its visceral system, and, as it grows, assumes a nature utterly unlike that which it originally had—a nature adapted almost exclusively to alimentation and the propagation of the species.

Let us turn now to the social organism, and the analogies of structure and function which may be traced in it. Of course these analogies between the phenomena presented in a physically coherent aggregate forming an individual, and the phenomena presented in a physically incoherent aggregate of individuals distributed over a wide area, cannot be analogies of a visible or sensible kind; but can only be analogies between the systems, or methods,

of organization. Such analogies as exist result from the one unquestionable community between the two organizations: *there is in both a mutual dependence of parts.* This is the origin of all organization; and determines what similarities there are between an individual organism and a social organism. Of course the similarities thus determined are accompanied by transcendent differences, determined, as above said, by the unlikenesses of the aggregates. One cardinal difference is that, while in the individual organism there is but one centre of consciousness capable of pleasure or pain, there are, in the social organism, as many such centres as there are individuals, and the aggregate of them has no consciousness of pleasure or pain —a difference which entirely changes the ends to be pursued. Bearing in mind this qualification, let us now glance at the parallelisms indicated.

A society, like an individual, has a set of structures fitting it to act upon its environment—appliances for attack and defence, armies, navies, fortified and garrisoned places. At the same time, a society has an industrial organization which carries on all those processes that make possible the national life. Though these two sets of organs for external activity and internal activity do not bear to one another just the same relation which the outer and inner organs of an animal do (since the industrial structures in a society supply themselves with raw materials, instead of being supplied by the external organs), yet they bear a relation otherwise similar. There is at once a coöperation and an antagonism. By the help of the defensive system the industrial system is enabled to carry on its functions without injury from foreign enemies; and by the help of the industrial system, which supplies it with food and materials, the defensive system is enabled to maintain this security. At the same time the two systems are opposed

in so far that they both depend for their existence upon the common stock of produce. Further, in the social organism, as in the individual organism, this primary coöperation and antagonism subdivides into secondary coöperations and antagonisms. If we look at the industrial organization, we see that its agricultural part and its manufacturing part aid one another by the exchange of their products, and are yet otherwise opposed to one another; since each takes of the other's products the most it can get in return for its own products. Similarly throughout the manufacturing system itself. Of the total returns secured by Manchester for its goods, Liverpool obtains as much as possible for the raw material, and Manchester gives as little as possible—the two at the same time coöperating in secreting for the rest of the community the woven fabrics it requires, and in jointly obtaining from the rest of the community the largest payment in other commodities. And thus it is in all kinds of direct and indirect ways throughout the industrial structures. Men prompted by their own needs as well as those of their children, and bodies of such men more or less aggregated, are quick to find every unsatisfied need of their fellow-men, and to satisfy it in return for the satisfaction of their own needs; and the working of this process is inevitably such that the strongest need, ready to pay the most for satisfaction, is that which draws most workers to satisfy it, so that there is thus a perpetual balancing of the needs and of the appliances which subserve them.

This brings us to the regulative structures under which these two systems of coöperating parts work. As in the individual organism, so in the social organism, the outer parts are under a rigorous central control. For adjustment to the varying and incalculable changes in the environment, the external organs, offensive and defensive,

must be capable of prompt combination; and that their actions may be quickly combined to meet each exigency as it arises, they must be completely subordinated to a supreme executive power—armies and navies must be despotically controlled. Quite otherwise is it with the regulative apparatus required for the industrial system. This, which carries on the nutrition of a society, as the visceral system carries on the nutrition of an individual, has a regulative apparatus in great measure distinct from that which regulates the external organs. It is not by any "order in council" that farmers are determined to grow so much wheat and so much barley, or to divide their land in due proportion between arable and pasture. There requires no telegram from the Home Office to alter the production of woollens in Leeds, so that it may be properly adjusted to the stocks on hand and the forthcoming crop of wool. Staffordshire produces its due quantity of pottery, and Sheffield sends out cutlery with rapidity adjusted to the consumption, without any legislative stimulus or restraint. The spurs and checks to production which manufacturers and manufacturing centres receive, have quite another origin. Partly by direct orders from distributors and partly by the indirect indications furnished by the market reports throughout the kingdom, they are prompted to secrete actively or to diminish their rates of secretion. The regulative apparatus by which these industrial organs are made to coöperate harmoniously, acts somewhat as the sympathetic does in a vertebrate animal. There is a system of communications among the great producing and distributing centres, which excites or retards as the circumstances vary. From hour to hour messages pass between all the chief provincial towns, as well as between each of them and London; from hour to hour prices are adjusted, supplies are ordered hither or

thither, and capital is drafted from place to place, according as there is greater or less need for it. All this goes on without any ministerial overseeing—without any dictation from those executive centres which combine the actions of the outer organs. There is, however, one all-essential influence which these higher centres exercise over the industrial activities—a restraining influence which prevents aggression, direct and indirect. The condition under which only these producing and distributing processes can go on healthfully is that, wherever there is work and waste, there shall be a proportionate supply of materials for repair. And securing this is nothing less than securing fulfilment of contracts. Just in the same way that a bodily organ which performs function, but is not adequately paid in blood, must dwindle, and the organism as a whole eventually suffer; so an industrial centre which has made and sent out its special commodity, but does not get adequately paid in other commodities, must decay. And when we ask what is requisite to prevent this local innutrition and decay, we find the requisite to be that agreements shall be carried out; the goods shall be paid for at the stipulated prices; that justice shall be administered.

One further leading parallelism must be described—that between the metamorphoses which occur in the two cases. These metamorphoses are analogous in so far that they are changes in the ratios of the inner and outer systems of organs; and also in so far as they take place under analogous conditions. At the one extreme we have that small and simple type of society which a wandering horde of savages presents. This is a type almost wholly predatory in its organization. It consists of little else than a co-operative structure for carrying on warfare—the industrial part is almost absent, being represented only by the women. When the wandering tribe becomes a settled tribe, an

industrial organization begins to show itself—especially where, by conquest, there has been obtained a slave-class that may be forced to labor. The predatory structure, however, still for a long time predominates. Omitting the slaves and the women, the whole body politic consists of parts organized for offence and defence, and is efficient in proportion as the control of them is centralized. Communities of this kind, continuing to subjugate their neighbors, and developing an organization of some complexity, may nevertheless retain a mainly-predatory type, with just such industrial structures as are needful for supporting the offensive and defensive structures. Of this Sparta furnished a good example. The characteristics of such a social type are these—that each member of the ruling race is a soldier; that war is the business of life; that every one is subject to a rigorous discipline fitting him for this business; that centralized authority regulates all the social activities, down to the details of each man's daily conduct; that the welfare of the State is every thing, and that the individual lives for public benefit. So long as the environing societies are such as necessitate and keep in exercise the predatory organization, these traits continue; but when, mainly by conquest and the formation of large aggregates, the predatory activity becomes less constant, and war ceases to be the occupation of every free man, the industrial structures begin to predominate. Without tracing the transition, it will suffice to take, as a sample of the pacific or industrial type, the Northern States of America before the late war. Here military organization had almost disappeared; the infrequent local assemblings of militia had turned into occasions for jollity, and every thing martial had fallen into contempt. The traits of the pacific or industrial type are these—that the central authority is relatively feeble; that it interferes

scarcely at all with the private actions of individuals; and that the State, instead of being that for the benefit of which individuals exist, has become that which exists for the benefit of individuals.

It remains to add that this metamorphosis, which takes place in societies along with a higher civilization, very rapidly retrogrades if the surrounding conditions become unfavorable to it. During the late war in America, Mr. Seward's boast—"I touch this bell, and any man in the remotest State is a prisoner of the Government" (a boast which was not an empty one, and which was by many of the Republican party greatly applauded)—shows us how rapidly, along with predatory activities, there tends to be resumed the needful type of centralized structure; and how there quickly grow up the corresponding sentiments and ideas. Our own history since 1815 has shown a double change of this kind. During the thirty years' peace, the predatory organization dwindled, the military sentiment greatly decreased, the industrial organization rapidly developed, the assertion of the individuality of the citizen became more decided, and many restrictive and despotic regulations were got rid of. Conversely, since the revival of predatory activities and structures on the Continent, our own offensive and defensive structures have been redeveloping, and the tendency toward increase of that centralized control which accompanies such structures has become marked.

And now, closing this somewhat elaborate introduction, I am prepared to deal with the question put to me. Prof. Huxley, after quoting some passages from that essay on the "Social Organism" which I have supplemented in the foregoing paragraphs; and after expressing a qualified concurrence which I greatly value as coming from so

highly fitted a judge, proceeds, with characteristic acumen, to comment on what seems an incongruity between certain analogies set forth in that essay, and the doctrine I hold respecting the duty of the State. Referring to a passage in which I have described the function of the individual brain as "that of *averaging* the interests of life, physical, intellectual, moral, social," and have compared it to the function of Parliament as "that of *averaging* the interests of the various classes in a community," adding that "a good Parliament is one in which the parties answering to these respective interests are so balanced that their united legislation concedes to each class as much as consists with the claims of the rest"—Prof. Huxley proceeds to say:

"All this appears to be very just. But if the resemblances between the body physiological and the body politic are any indication, not only of what the latter is, and how it has become what it is, but what it ought to be, and what it is tending to become, I cannot but think that the real force of the analogy is totally opposed to the negative view of State function.

"Suppose that, in accordance with this view, each muscle were to maintain that the nervous system had no right to interfere with its contraction, except to prevent it from hindering the contraction of another muscle; or each gland, that it had a right to secrete, so long as its secretion interfered with no other; suppose every separate cell left free to follow its own "interest," and *laissez-faire* Lord of all, what would become of the body physiological?"

On this question the remark I have first to make is, that if I held the doctrine of M. Proudhon, who deliberately named himself an "anarchist," and if along with this doctrine I held the above-indicated theory of social structures and functions, the inconsistency implied by the question put would be clear, and the question would be unanswerable. But since I entertain no such view as that of Proudhon—since I hold that within its proper limits governmental action is not simply legitimate but all-important

—I do not see how I am concerned with a question which tacitly supposes that I deny the legitimacy and the importance. Not only do I contend that the restraining power of the State over individuals, and bodies or classes of individuals, is requisite, but I have contended that it should be exercised much more effectually, and carried out much further, than at present.* And as the maintenance of this control implies the maintenance of a controlling apparatus, I do not see that I am placed in any difficulty when I am asked what would happen were the controlling apparatus forbidden to interfere. Further, on this general aspect of the question I have to add that, by comparing the deliberative assembly of a nation to the deliberative nervous centre of a vertebrate animal, as respectively averaging the interests of the society and of the individual, and as both doing this through processes of representation, I do not mean to *identify* the two sets of interests; for these in a society (or at least a peaceful society) refer mainly to interior actions, while in an individual creature they refer mainly to exterior actions. The "interests" to which I refer, as being averaged by a representative governing body, are the conflicting interests between class and class, as well as between man and man—conflicting interests the balancing of which is nothing but the preventing of aggression and the administration of justice.

I pass now from this general aspect of the question, which does not concern me, to a more special aspect which does concern me. Dividing the actions of governing structures, whether in bodies individual or bodies politic, into the *positively regulative* and the *negatively regulative*, or those which stimulate and direct, as distinguished from those which simply restrain, I may say that if there is raised the question—What will happen

* *See* "Social Statics," chap. xxi., "The Duty of the State."

when the controlling apparatus does not act? there are quite different replies according as one or other system of organs is referred to. If, in the individual body, the muscles were severally independent of the deliberative and executive centres, utter impotence would result: in the absence of muscular coördination, there would be no possibility of standing, much less of acting on surrounding things, and the body would be a prey to the first enemy. Properly to combine the actions of these outer organs, the great nervous centres must exercise functions that are both positively regulative and negatively regulative—must both command action and arrest action. Similarly with the outer organs of a political body. Unless the offensive and defensive structures can be despotically commanded by a central authority, there cannot be those prompt combinations and adjustments required for meeting the variable actions of external enemies. But if, instead of asking what would happen supposing the outer organs in either case were without control from the great governing centres, we ask what would happen were the inner organs (the industrial and commercial structures in the one case, and the alimentary and distributive in the other) without such control, the answer is quite different. Omitting the respiratory and some minor ancillary parts of the individual organism, to which the social organism has nothing analogous; and limiting ourselves to absorptive, elaborative, and distributive structures, which are found in both; it may, I think, be successfully contended that in neither the one case nor the other do they require the positively regulative control of the great governing centres, but only the negatively regulative. Let us glance at the facts.*

* Lest there should be any misunderstanding of the terms *positively regulative* and *negatively regulative*, let me briefly illustrate them. If a man has land, and neither cultivate it for him, partially or wholly, or dictate any or

Digestion and circulation go on very well in lunatics and idiots, though the higher nervous centres are either deranged or partly absent. The vital functions proceed properly during sleep, though less actively than when the brain is at work. In infancy, while the cerebro-spinal system is almost incapable, and cannot even perform such simple actions as those of commanding the sphincters, the visceral functions are active and regular. Nor in an adult does that arrest of cerebral action shown by insensibility, or that extensive paralysis of the spinal system which renders all the limbs immovable, prevent these functions from being carried on for a considerable time; though they necessarily begin to flag in the absence of the demand which an active system of outer organs makes upon them. These internal organs are, indeed, so little under the positively directive control of the great nervous centres, that their independence is often very inconvenient. No mandate sent into the interior stops an attack of diarrhœa; nor, when an indigestible meal excites the circulation at night, and prevents sleep, will the bidding of the brain cause the heart to pulsate more quietly. It is doubtless true that these vital processes are modified in important ways, both by general stimulation and by inhibition, from the cerebro-spinal system; but that they are mainly independent cannot, I think, be questioned. The facts that peristaltic motion of the intestines can go on when their nervous connections are cut, and that the

all of his modes of cultivation, my action is positively regulative; but, if, leaving him absolutely unhelped and unregulated in his farming, I simply prevent him from taking his neighbor's crops, or from making approach-roads over his neighbor's land, or from depositing rubbish upon it, my action is negatively regulative. There is a tolerably sharp distinction between the act of securing a citizen's ends for him or interfering with his mode of securing them, and the act of checking him when he interferes with another citizen in the pursuit of his ends.

heart (in cold-blooded vertebrates, at least) continues to pulsate for some time after being detached from the body, make it manifest that the spontaneous activities of these vital organs subserve the wants of the body at large without direction from its higher governing centres. And this is made even more manifest if it be a fact, as alleged by Schmulewitsch experimenting under Ludwig's direction, that, under duly-adjusted conditions, the secretion of bile may be kept up for some time when blood is passed through the excised liver of a newly-killed rabbit. There is an answer, not, I think, unsatisfactory, even to the crucial part of the question—"Suppose every separate cell left free to follow its own interests, and *laissez-faire* Lord of all, what would become of the body physiological?" Limiting the application of this question in the way above shown to the organs and parts of organs which carry on vital actions, it seems to me that much evidence may be given for the belief that, when they follow their respective "interests" (limited here to growing and multiplying), the general welfare will be tolerably well secured. It was proved by Hunter's experiments on a kite and a sea-gull, that a part of the alimentary canal which has to triturate harder food than that which the creature naturally eats, acquires a thicker and harder lining. When a stricture of the intestine impedes the passage of its contents, the muscular walls of the intestine above, thicken and propel the contents with greater force. When there is somewhere in the course of a circulation a serious resistance to the passage of blood, there habitually occurs hypertrophy of the heart, or thickening of its muscular walls; giving it greater power to propel the blood. And similarly, when the duct through which it discharges its contents is obstructed, the gall-bladder thickens and strengthens. These changes go on without any direction

from the brain—without any consciousness that they are going on. They are effected by the growth, or multiplication, or adaptation, of the local units, be they cells or fibres, which results from the greater action or modified action thrown upon them. The only prerequisite to this spontaneous adaptive change is, that these local units shall be supplied with extra blood in proportion as they perform extra function—a prerequisite answering to that secured by the administration of justice in a society; namely, that more work shall bring more pay. If, however, direct proof be called for that a system of organs may, by carrying on their several independent activities uncontrolled, secure the welfare of the aggregate they form, we have it in that extensive class of creatures which do not possess any nervous systems at all; and which nevertheless show, some of them, considerable degrees of activity. The Oceanic Hydrozoa supply good examples. Notwithstanding "the multiplicity and complexity of the organs which some of them possess," these creatures have no nervous centres—no regulative apparatus by which the actions of their organs are coördinated. One of their higher kinds is composed of different parts distinguished as cœnosarc, polypites, tentacles, hydrocysts, nectocalyces, genocalyces, etc., and each of these different parts is composed of many partially-independent units—thread-cells, ciliated cells, contractile fibres, etc.; so that the whole organism is a group of heterogeneous groups, each one of which is itself a more or less heterogeneous group. And, in the absence of a nervous system, the arrangement must necessarily be such that these different units, and different groups of units, severally pursuing their individual lives without positive direction from the rest, nevertheless do, by virtue of their constitutions, and the relative positions into which they have grown, coöperate for the mainte-

nance of one another and the entire aggregate. And if this can be so with a set of organs that are not connected by nerves, much more can it be so with a set of organs which, like the viscera of a higher animal, have a special set of nervous communications for exciting one another to coöperation.

Let us turn now to the parallel classes of phenomena which the social organism presents. In it, as in the individual organism, we find that while the system of external organs must be rigorously subordinated to a great governing centre which positively regulates it, the system of internal organs needs no such positive regulation. The production and interchange by which the national life is maintained, go on as well while Parliament is not sitting as while it is sitting. When the members of the Ministry are following grouse or stalking deer, Liverpool imports, Manchester manufactures, London distributes, just as usual. All that is needful for the normal performance of these internal social functions is, that the restraining or inhibitory structures shall continue in action: these activities of individuals, corporate bodies, and classes, must be carried on in such ways as not to transgress certain conditions necessitated by the simultaneous carrying on of other activities. So long as order is maintained, and the fulfilment of contracts is everywhere enforced — so long as there is secured to each citizen, and each combination of citizens, the full return agreed upon for work done or commodities produced; and so long as each may enjoy what he obtains by labor, without trenching on his neighbor's like ability to enjoy; these functions will go on healthfully—more healthfully, indeed, than when regulated in any other way. Fully to recognize the fact, it is needful only to look at the origins and actions of the leading industrial structures. We will take two of them, the most remote from one another in their natures.

The first shall be those by which food is produced and distributed. In the fourth of his "Introductory Lectures on Political Economy," Archbishop Whately remarks that:

> "Many of the most important objects are accomplished by the joint agency of persons who never think of them, nor have any idea of acting in concert; and that, with a certainty, completeness, and regularity, which probably the most diligent benevolence, under the guidance of the greatest human wisdom, could never have attained."

To enforce this truth he goes on to say: "Let any one propose to himself the problem of supplying with daily provisions of all kinds such a city as our metropolis, containing above a million of inhabitants." And then he points out the many immense difficulties of the task caused by inconstancy in the arrival of supplies; by the perishable nature of many of the commodities; by the fluctuating number of consumers; by the heterogeneity of their demands; by variations in the stocks, immediate and remote, and the need for adjusting the rate of consumption; and by the complexity in the process of distribution, required to bring due quantities of these many commodities to the homes of all citizens. And, having dwelt on these many difficulties, he finishes his picture by saying:

> "Yet this object is accomplished far better than it could be by any effort of human wisdom, through the agency of men, who think each of nothing beyond his own immediate interest—who, with that object in view, perform their respective parts with cheerful zeal—and combine unconsciously to employ the wisest means for effecting an object, the vastness of which it would bewilder them even to contemplate."

But though the far-spreading and complex organization by which foods of all kinds are produced, prepared, and distributed throughout the entire kingdom, is a natural growth and not a State manufacture; though the State

does not determine where and in what quantities cereals and cattle and sheep shall be reared; though it does not arrange their respective prices so as to make supplies last until fresh supplies can come; though it has done nothing toward causing that great improvement of quality which has taken place in food since early times; though it has not the credit of that elaborate apparatus by which bread, and meat, and milk, and groceries, come round to our doors with a daily pulse that is as regular as the pulse of the heart; yet the State has not been wholly passive. It has from time to time done a great deal of mischief. When Edward I. forbade all towns to harbor forestallers, and when Edward VI. made it penal to buy grain for the purpose of selling it again, they were preventing the process by which consumption is adjusted to supply; they were doing all that could be done to insure alternations of abundance and starvation. Similarly with the many legislative attempts since made to regulate one branch or other of the food-industry, down to the corn-law sliding-scale of odious memory. For the marvellous efficiency of this organization we are indebted to private enterprise; while the derangements of it we owe to the positively-regulative action of the Government. Meanwhile, the negatively-regulative action required to keep this organization in order, Government has not duly performed. A quick and costless remedy for breach of contract, when a trader sells, as the commodity asked for, what proves to be wholly or in part some other commodity, is still wanting.

Our second case shall be the organization which so immensely facilitates commerce by transfers of claims and credits. Banks were not inventions of rulers or their counsellors. They grew up by small stages out of the transactions of traders with one another. Men who for security deposited money with goldsmiths, and took re-

ceipts; goldsmiths who began to lend out at interest the moneys left with them, and then to offer interest at lower rates to those who would deposit money; were the founders of them. And when, as presently happened, the receipt-notes became transferable by indorsement, banking commenced. From that stage upward the development, notwithstanding many hinderances, has gone on naturally. Banks have sprung up under the same stimulus which has produced all other kinds of trading bodies; the multiplied forms of credit have been gradually differentiated from the original form; and while the banking system has spread and become complex, it has also become consolidated into a whole by a spontaneous process. The clearing-house, which is a place for carrying on the banking between bankers, arose unobtrusively out of the effort to economize time and money. And when, in 1862, Sir John Lubbock—not in his legislative capacity, but in his capacity as banker—succeeded in extending the privileges of the clearing-house to country banks, the unification was made perfect; so that now the transactions of any trader in the kingdom with any other may be completed by the writing off and balancing of claims in bankers' books. This natural evolution, be it observed, has reached with us a higher phase than has been reached where the positively-regulative control of the State is more decided. They have no clearing-house in France; and, in France, the method of making payments by checks, so dominant among ourselves, is very little employed and in an imperfect way. I do not mean to imply that in England the State has been a mere spectator of this development. Unfortunately, it has from the beginning had relations with banks and bankers: not much, however, to their advantage, or that of the public. The first kind of deposit-bank was in some sense a State-bank; merchants left funds

for security at the Mint in the Tower. But when Charles I. appropriated their property without consent, and gave it back to them only under pressure, after a long delay, he destroyed their confidence. Similarly, when Charles II., in furtherance of State-business, came to have habitual transactions with the richer of the private bankers; and when, having got nearly a million and a half of their money in the Exchequer, he stole it, ruined a multitude of merchants, distressed ten thousand depositors, and made some lunatics and suicides, he gave a considerable shock to the banking system as it then existed. Though the results of State-relations with banks in later times have not been so disastrous in this direct way, yet they have been indirectly disastrous—perhaps even in a greater degree. In return for a loan, the State gave the Bank of England special privileges; and for the increase and continuance of this loan the bribe was the maintenance of these privileges—privileges which immensely hindered the development of banks. The State did worse—it led the Bank of England to the verge of bankruptcy by a forced issue of notes, and then authorized it to break its promises to pay. Nay, worse still, it prevented the Bank of England from fulfilling its promises to pay when it wished to fulfil them. The evils that have arisen from the positively-regulative action of the State on banks are too multitudinous to be here enumerated. They may be found in the writings of Tooke, Newmarch, Fullarton, Macleod, Wilson, J. S. Mill, and others. All we have here to note is, that while the enterprise of citizens in the pursuit of private ends has developed this great trading-process, which so greatly facilitates all other trading-processes, Governments have over and over again disturbed it to an almost fatal extent; and that, while they have done immense mischief of one kind by their positively-

regulative action, they have done immense mischief of another kind by failing in their negatively-regulative action. They have not done the one thing they had to do: they have not uniformly insisted on fulfilment of contract between the banker and the customer who takes his promise to pay on demand.

Between these two cases of the trade in food and the trade in money might be put the cases of other trades—all of them carried on by organizations similarly evolved, and similarly more or less deranged from time to time by State-meddling. Passing over these, however, let us turn from the positive method of elucidation to the comparative method. When it is questioned whether the spontaneous coöperation of men in pursuit of personal benefits will adequately work out the general good, we may get guidance for judgment by comparing the results achieved in societies where spontaneous coöperation has been most active and least regulated, with the results achieved in societies where spontaneous coöperation has been less trusted and State-action more trusted. Two cases, furnished by the two leading nations on the Continent, will suffice:

In France, the École des Ponts et Chaussées was founded in 1747 for educating civil engineers; and in 1795 was founded the École Polytechnique, serving, among other purposes, to give a general scientific training to those who were afterward to be more specially trained for civil engineering. Averaging the two dates, we may say that for a century France has had a State-established and State-maintained appliance for producing skilled men of this class—a double gland, we may call it, to secrete engineering faculty for public use. In England, until quite recently, we have had no institution for preparing civil engineers. Not by intention, but unconsciously, we left the furnishing of engineering faculty to take place

under the law of supply and demand—a law which at present seems to be no more recognized as applying to education, than it was recognized as applying to commerce in the days of bounties and restrictions. This, however, by the way. We have here simply to note that Brindley, Smeaton, Rennie, Telford, and the rest, down to George Stephenson, acquired their knowledge, and got their experience, without State aid or supervision. What have been the comparative results in the two nations? Space does not allow a detailed comparison: the later results must suffice. Railways originated in England, not in France. Railways spread through England faster than through France. Many railways in France were laid out and officered by English engineers. The earlier French railways were made by English contractors; and English locomotives served the French makers as models. The first French work written on locomotive engines, published about 1840 (at least I had a copy at that date), was by the Comte de Pambour, who had studied in England, and who gave in his work nothing whatever but drawings and descriptions of the engines of English makers.

The second illustration is supplied to us by the model nation, now so commonly held up to us for imitation. Let us contrast London and Berlin in respect of an all-essential appliance for the comfort and health of citizens. When, at the beginning of the seventeenth century, the springs and local conduits, supplemented by water-carriers, failed to supply the Londoners; and when the water-famine, for a long time borne, had failed to make the Corporation do more than propose schemes, and had not spurred the central government to do any thing; Hugh Myddleton, a merchant citizen, took in hand himself the work of bringing the New River to Islington. When he had half-completed the work, the king came to his help—not, in-

deed, in his capacity of ruler, but in the capacity of speculator, investing his money with a view to profit: his share being disposed of by his successor after the formation of the New River Company, which finished the distributing system. Subsequently, the formation of other water-companies, utilizing other sources, has given London a water-supply that has grown with its growth. What, meanwhile, happened at Berlin? Did there in 1613, when Hugh Myddleton completed his work, grow up there a like efficient system? Not at all. The seventeenth century passed, the eighteenth century passed, the middle of the nineteenth century was reached, and still Berlin had no water-supply like that of London. What happened then? Did the paternal government at length do what had been so long left undone? No. Did the citizens at length unite to secure the desideratum? No. It was finally achieved by the citizens of another nation, more accustomed to coöperate in securing their own profits by ministering to public needs. In 1845 an English company was formed for giving Berlin an adequate water-supply; and the work was executed by English contractors—Messrs. Fox and Crampton.

Should it be said that great works of ancient nations, in the shape of aqueducts, roads, etc., might be instanced in proof that State agency secures such ends, or should it be said that a comparison between the early growth of inland navigation on the Continent, and its later growth here, would be to our disadvantage, I reply that, little as they at first seem so, these facts are congruous with the general doctrine. While the predatory social type is dominant, and the industrial organization but little developed, there is but one coördinating agency for regulating both sets of activities, just as we saw happens with the lower types of individual organisms. It is only when a consid-

erable advance has been made in that metamorphosis which develops the industrial structures at the expense of the predatory structures, and which brings along with it a substantially independent coördinating agency for the industrial structures—it is only then that the efficiency of these spontaneous coöperations for all purposes of internal social life becomes greater than the efficiency of the central governing agency.

Possibly it will be said that, though, for subserving material needs, the actions of individuals, stimulated by necessity and made quick by competition, are demonstrably adequate, they are not adequate for subserving other needs. I do not see, however, that the facts justify this position. We have but to glance around to find in abundance similarly-generated appliances for satisfying our higher desires, as well as our lower desires. The fact that the Fine Arts have not thriven here as much as in some Continental countries is ascribable to natural character, to absorption of our energies in other activities, and to the repressive influence of chronic asceticism, rather than to the absence of fostering agencies: these the interests of individuals have provided in abundance. Literature, in which we are second to none, owes, with us, nothing to State aid. The poetry which will live is poetry which has been written without official prompting, and, though we have habitually had a prize-poet, paid to write loyal verses, it may be said, without disparaging the present one, that a glance over the entire list does not show any benefit derived by poetry from State patronage. Nor are other forms of literature any more indebted to State patronage. It was because there was a public liking for fiction that fiction began to be produced, and the continued public liking causes a continued production, including, along with much that is worthless, much that could not have

been made better by any academic or other supervision. And the like holds of biographies, histories, scientific books, etc. Or, as a still more striking case of an agency that has grown up to meet a non-material want, take the newspaper press. What has been the genesis of this marvellous appliance, which each day gives us an abstract of the world's life the day before? Under what promptings have there been got together its staffs of editors, sub-editors, article-writers, reviewers; its reporters of parliamentary debates, of public meetings, of law cases and police cases; its critics of music, theatricals, paintings, etc.; its correspondents in all parts of the world? Who devised and brought to perfection this system which at six o'clock in the morning gives the people of Edinburgh a report of the debates that ended at two or three o'clock in the House of Commons, and at the same time tells them of events that occurred the day before in America? It is not a Government invention. It is not a Government suggestion. It has not been in any way improved or developed by legislation. On the contrary, it has grown up in spite of many hinderances from the Government, and burdens which the Government has imposed on it. For a long time the reporting of parliamentary debates was resisted; for generations censorships and prosecutions kept newspapers down, and for several subsequent generations the laws in force negatived a cheap press, and the educational benefits accompanying it. From the war-correspondent, whose letters give to the very nations that are fighting their only trustworthy accounts of what is being done, down to the newsboy who brings round the third edition with the latest telegrams, the whole organization is a product of spontaneous coöperation among private individuals, aiming to benefit themselves by ministering to the intellectual needs of their fellows—aiming also, not a

few of them, to benefit their fellows by giving them clearer ideas and a higher standard of right. Nay, more than this is true. While the press is not indebted to the Government, the Government is enormously indebted to the press, without which, indeed, it would stumble daily in the performance of its functions. This agency, which the State once did its best to put down, and has all along impeded, now gives to the ministers news in anticipation of their dispatches, gives to members of Parliament a guiding knowledge of public opinion, and enables them to speak from the House of Commons benches to their constituents, and gives to both legislative chambers a full record of their proceedings.

I do not see, therefore, how there can be any doubt respecting the sufficiency of agencies thus originating. The truth, that, in this condition of mutual dependence brought about by social life, there inevitably grow up arrangements such that each secures his own ends by ministering to the ends of others, seems to have been for a long time one of those open secrets which remain secret because they are so open; and even now the conspicuousness of this truth seems to cause an imperfect consciousness of its full meaning. The evidence shows, however, that, even were there no other form of spontaneous coöperation among men than that dictated by self-interest, it might be rationally held that this, under the negatively-regulative control of a central power, would work out, in proper order, the appliances for satisfying all needs, and carrying on healthfully all the essential social functions.

But there is a further kind of spontaneous coöperation, arising, like the other, independently of State action, which takes a large share in satisfying certain classes of needs. Familiar though it is, this kind of spontaneous cooperation is habitually ignored in sociological discussions.

Alike from newspaper articles and parliamentary debates, it might be inferred that, beyond the force due to men's selfish activities, there is no other social force than the governmental force. There seems to be a deliberate omission of the fact that, in addition to their selfish interests, men have sympathetic interests, which, acting individually and coöperatively, work out results scarcely less remarkable than those which the selfish interests work out. It is true that, during the earlier phases of social evolution, while yet the type is mainly predatory, agencies thus produced do not exist: among the Spartans, I suppose, there were few, if any, philanthropic agencies. But as there arise forms of society leading toward the pacific type—forms in which the industrial organization develops itself, and men's activities become of a kind that do not perpetually sear their sympathies—these structures which their sympathies generate become many and important. To the egoistic interests, and the coöperations prompted by them, there come to be added the altruistic interests and their coöperations; and, what the one set fails to do, the other does. That, in his presentation of the doctrine he opposes, Prof. Huxley did not set down the effects of fellow-feeling as supplementing the effects of self-regarding feelings, surprises me the more, because he displays fellow-feeling himself in so marked a degree, and shows in his career how potent a social agency it becomes. Let us glance rapidly over the results wrought out among ourselves by individual and combined "altruism"—to employ M. Comte's useful word.

Though they show a trace of this feeling, I will not dwell upon the numerous institutions by which men are enabled to average the chances throughout life by insurance societies, which provide against the evils entailed by premature deaths, accidents, fires, wrecks, etc., for these

are mainly mercantile and egoistic in their origin. Nor will I do more than name those multitudinous Friendly Societies that have arisen spontaneously among the working-classes to give mutual aid in time of sickness, and which the Commission now sitting is showing to be immensely beneficial, notwithstanding their defects; for these also, though containing a larger element of sympathy, are prompted chiefly by anticipations of personal benefits. Leaving these, let us turn to the organizations in which altruism is more decided—taking first that by which religious ministrations are carried on. Throughout Scotland and England, cut away all that part of it which is not established by law—in Scotland, the Episcopal Church, the Free Church, the United Presbyterians, and other Dissenting bodies; in England, the Wesleyans, Independents, and the various minor sects. Cut off, too, from the Established Church itself, all that part added in recent times by voluntary zeal, made conspicuous enough by the new steeples that have been rising on all sides; and then also take out, from the remainder of the Established Church, that energy which has during these three generations been infused into it by competition with the Dissenters: so reducing it to the degraded, inert state in which John Wesley found it. Do this, and it becomes manifest that more than half the organization, and immensely more than half its function, is extra-governmental. Look round, again, at the multitudinous institutions for mitigating men's ills—the hospitals, dispensaries, almshouses, and the like—the various benevolent and mendicity societies, etc., of which London alone contains between six and seven hundred. From our vast St. Thomas's, exceeding the palace of the Legislature itself in bulk, down to Dorcas societies, and village clothing-clubs, we have charitable agencies, many in kind and

countless in number, which supplement, perhaps too largely, the legally-established one, and which, whatever evil they may have done along with the good, have done far less evil than the Poor-Law organization did before it was reformed in 1834. Akin to these are still more striking examples of power in agencies thus originating, such as that furnished by the Antislavery Society, which carried the emancipation of the slaves, notwithstanding the class opposition so predominant in the Legislature. And, if we look for more recent like instances, we have them in the organization which promptly and efficiently dealt with the cotton-famine in Lancashire, and in that which last year ministered to the wounded and distressed in France. Once more, consider our educational system as it existed till within these few years. Such part of it as did not consist of private schools, carried on for personal profit, consisted of schools or colleges set up or maintained by men for the benefit of their fellows, and the posterity of their fellows. Omitting the few founded or partially founded by kings, the numerous endowed schools scattered throughout the kingdom originated from altruistic feelings (so far, at least, as they were not due to egoistic desires for good places in the other world). And then, after these appliances for teaching the poor had been almost entirely appropriated by the rich, whence came the remedy? Another altruistic organization grew up for educating the poor, struggled against the opposition of the Church and the governing classes, eventually forced these to enter into competition, and produce like altruistic organizations, until by school systems, local and general, ecclesiastical, dissenting, and secular, the mass of the people had been brought from a state of almost entire ignorance to one in which nearly all of them possessed the rudiments of knowledge. But for these spontaneously,

developed agencies, ignorance would have been universal. Not only such knowledge as the poor now possess, not only the knowledge of the trading-classes, not only the knowledge of those who write books and articles for the press, but the knowledge of those who carry on the business of the country as ministers and legislators, has been derived from these extra-governmental agencies, egoistic or altruistic. Yet now, strangely enough, the cultured intelligence of the country has taken to spurning its parent; and that to which it owes both its existence and the consciousness of its own value is pooh-poohed as though it had done, and could do, nothing of importance. One other fact let me add: While such teaching organizations, and their results in the shape of enlightenment, are due to these spontaneous agencies, to such agencies also are due the great improvements in the quality of the culture now happily beginning to take place. The spread of scientific knowledge, and of the scientific spirit, has not been brought about by laws and officials. Our scientific societies have arisen from the spontaneous coöperation of those interested in the accumulation and diffusion of the kinds of truth they respectively deal with. Though the British Association has from time to time obtained certain small subsidies, their results in the way of advancing science have borne but an extremely small ratio to the results achieved without any such aid. If there needs a conclusive illustration of the power of agencies thus arising, we have it in the history and achievements of the Royal Institution. From this, which is a product of altruistic coöperation and which has had for its successive professors Young, Davy, Faraday, and Tyndall, there has come a series of brilliant discoveries which it would be difficult to parallel by a series from any State-nurtured institution.

I hold, then, that forced, as men in society are, to seek satisfaction of their own wants by saisfying the wants of others; and led as they also are by sentiments which social life has fostered, to satisfy many wants of others irrespective of their own; they are moved by two sets of forces which, working together, will amply suffice to carry on all needful activities; and I think the facts fully justify this belief. It is true that, *a priori*, one would not have supposed that by their unconscious coöperations men could have wrought out such results, any more than one would have supposed, *a priori*, that by their unconscious coöperation they, could have evolved Language. But reasoning *a posteriori*, which it is best to do when we have the facts before us, it becomes manifest that they can do this; that they have done it in very astonishing ways; and perhaps they may do it hereafter in ways still more transcending expectation. Scarcely any scientific generalization has, I think, a broader inductive basis than we have for the belief that these egoistic and altruistic feelings are powers which, taken together, amply suffice to originate and carry on all the activities which constitute healthy national life: the only prerequisite being, that they shall be under the negatively-regulative control of a central power—that the entire aggregate of individuals, acting through the legislature and executive as its agents, shall put upon each individual, and group of individuals, the restraints needful to prevent aggression, direct and indirect.

And here I might go on to supplement the argument by showing that the immense majority of the evils which government aid is invoked to remedy, are evils which arise immediately or remotely because it does not perform properly its negatively-regulative function. From the waste of, probably, £100,000,000 of national capital in unproductive railways, for which the Legislature is respon-

sible by permitting the original proprietary contracts to be broken,* down to the railway accidents and loss of life caused by unpunctuality, which would never have grown to its present height were there an easy remedy for breach of contract between company and passenger; nearly all the vices of railway management have arisen from the non-administration of justice. And everywhere else we shall find that, were the restraining action of the State prompt, effective, and costless to those aggrieved, the pleas put in for positive regulation would nearly all disappear.

I am thus brought naturally to remark on the title given to this theory of State functions. That "Administrative Nihilism" adequately describes the view set forth by Von Humboldt, may be: I have not read his work. But I cannot see how it adequately describes the doctrine I have been defending; nor do I see how this can be properly expressed by the more positive title, "police-government." The conception suggested by police-government does not include the conception of an organization for external protection. So long as each nation is given to burglary, I quite admit each other nation must keep guards, under the forms of army or navy, or both, to prevent burglars from breaking in. And the title police-government does not in its ordinary acceptation comprehend these offensive and defensive appliances needful for dealing with foreign enemies. At the other extreme, too, it falls short of the full meaning to be expressed. While it duly conveys the idea of an organization required for checking and punishing criminal aggression, it does not convey any idea of the no less important organization required for dealing with civil aggression—an organization

* See Essay on "Railway Morals and Railway Policy."

quite essential for properly discharging the negatively-regulative function. Though latent police-force may be considered as giving their efficiency to legal decisions on all questions brought into *nisi prius* courts, yet, since here police-force rarely comes into visible play, police-government does not suggest this very extensive part of the administration of justice. Far from contending for a *laissez-faire* policy in the sense which the phrase commonly suggests, I have contended for a more active control of the kind distinguishable as negatively regulative. One of the reasons I have urged for excluding State action from other spheres, is, that it may become more efficient within its proper sphere. And I have urged that the wretched performance of its duties within its proper sphere continues, because it is mainly occupied with other duties.* The facts that often, in bankruptcy cases, three-fourths and more of the assets go in costs; that creditors are led by the expectation of great delay and a miserable dividend to accept almost any composition offered; and that so the bankruptcy-law offers a premium to roguery; are facts which would long since have ceased to be facts, had citizens been mainly occupied in getting an efficient judicial system. If the due performance by the State of its all-essential function had been the question on which elections were fought, we should not see, as we now do, that a shivering cottager who steals palings for firewood, or a hungry tramp who robs an orchard, gets punishment in more than the old Hebrew measure, while great financial frauds which ruin their thousands bring no punishments. Were the negatively-regulative function of the State in internal affairs dominant in the thoughts of men, within the Legislature and without, there would be tolerated no such treatment as that suffered lately by Messrs. Walker, of Corn-

* *See* Essay on "Over-Legislation."

hill; who, having been robbed of £6,000 worth of property and having spent £950 in rewards for apprehending thieves, and in prosecuting them, cannot get back the proceeds of their property found on the thieves—who bear the costs of administering justice, while the Corporation of London makes £940 profit out of their loss. It is in large measure because I hold that these crying abuses and inefficiencies, which everywhere characterize the administration of justice, need more than any other evils to be remedied; and because I hold that remedy of them can go on only as fast as the internal function of the State is more and more restricted to the administration of justice; that I take the view which I have been reëxplaining. *It is a law universally illustrated by organizations of every kind, that, in proportion as there is to be efficiency, there must be specialization, both of structure and function—specialization which, of necessity, implies accompanying limitation.* And, as I have elsewhere argued, the development of representative government is the development of a type of government fitted above all others for this negatively-regulative control, and, above all others, ill fitted for positively-regulative control.* This doctrine, that while the negatively-regulative control should be extended and made better, the positively-regulative control should be diminished, and that the one change implies the other, may be properly called the doctrine of Specialized Administration—if it is to be named from its administrative aspect. I regret that my presentation of this doctrine has been such as to lead to misinterpretation. Either it is that I have not adequately explained it, which, if true, surprises me, or else it is that the space occupied in seeking to show what are not the duties of the State is so much greater than the space occupied in defining its

* *See* Essay on "Representative Government what is it good for?"

duties, that these last make but little impression. In any case, that Prof. Huxley should have construed my view in the way he has done, shows me that it needs fuller exposition; since, had he put upon it the construction I intended, he would not, I think, have included it under the title he has used, nor would he have seen it needful to raise the question I have endeavored to answer.

POSTSCRIPT.—Since the above article was written, a fact of some significance in relation to the question of State-management has come under my notice. There is one department, at any rate, in which the State succeeds well—the Post-Office. And this department is sometimes instanced as showing the superiority of public over private administration.

I am not about to call in question the general satisfactoriness of our postal arrangements; nor shall I contend that this branch of State-organization, now well-established, could be replaced with advantage. Probably the type of our social structure has become, in this respect, so far fixed that a radical change would be injurious. In dealing with those who make much of this success, I have contented myself with showing that the developments which have made the Post-Office efficient, have not originated with the Government, but have been thrust upon it from without. I have in evidence cited the facts that the mail-coach system was established by a private individual, Mr. Palmer, and lived down official opposition; that the reform originated by Mr. Rowland Hill had to be made against the wills of *employés;* and, further, I have pointed out that, even as it is, a large part of the work is done by private enterprise—that the Government gets railway-companies to do for it most of the inland carriage, and steam-boat companies the

outland carriage: contenting itself with doing the local collection and distribution.

Respecting the general question whether, in the absence of our existing postal system, private enterprise would have developed one as good or better, I have been able to say only that analogies like that furnished by our newspaper-system, with its efficient news-vending organization, warrant us in believing that it would. Recently, however, I have been shown both that private enterprise is capable of this, and that, but for a legal interdict, it would have done long ago what the State has but lately done. Here is the proof:—

"To facilitate correspondence between one part of London and another was not originally one of the objects of the Post-Office. But, in the reign of Charles II., an enterprising citizen of London, William Dockwray, set up, at great expense, a penny post, which delivered letters and parcels six or eight times a-day in the busy and crowded streets near the Exchange, and four times a-day in the outskirts of the capital. . . . As soon as it became clear that the speculation would be lucrative, the Duke of York complained of it as an infraction of his monopoly, and the courts of law decided in his favour."—*Macaulay*, i. 387-8.

Thus it appears that two centuries since, private enterprise initiated a local postal system, similar, in respect both of cheapness and frequency of distribution, to that lately-established one boasted of as a State-success. Judging by what has happened in other cases with private enterprises that had small beginnings, we may infer that the system thus commenced, would have developed throughout the kingdom as fast as the needs pressed and the possibilities allowed. So far from being indebted to the State, we have reason to believe that, but for State-repression, we should have obtained a postal organization like our present one generations ago!

VI.

"THE COLLECTIVE WISDOM."

[FROM THE READER FOR APRIL 15, 1865.]

"THE COLLECTIVE WISDOM."

A TEST of senatorial capacity is a desideratum. We rarely learn how near the mark or how wide of the mark the calculations of statesmen are; the slowness and complexity of social changes, hindering, as they do, the definite comparison of results with anticipations. Occasionally, however, parliamentary decisions admit of being definitely valued. One which was arrived at a few weeks ago furnished a measure of legislative judgment too significant to be passed by.

On the edge of the Cotswolds, overhanging the valley of the Severn, occur certain springs, which, as they happen to be at the end of the longest of the hundred streams which join to form the Thames, have been called by a poetical fiction "the sources of the Thames." Names, even when poetical fictions, suggest conclusions; and conclusions drawn from words instead of facts are equally apt to influence conduct. Thus it happened that, when, recently, there was formed a company for supplying Cheltenham and some other places from these springs, great opposition arose. The *Times* published a paragraph, headed, "Threatened Absorption of the Thames," stating that the application of this company to Parliament had "caused

some little consternation in the city of Oxford, and will, doubtless, throughout the valley of the Thames;" and that "such a measure, if carried out, will diminish the water of that noble river a million of gallons per day." A million is an alarming word—suggests something necessarily vast. Translating words into thoughts, however, would have calmed the fears of the *Times* paragraphist. Considering that a million gallons would be contained by a room fifty-six feet cube, the nobility of the Thames would not be much endangered by the deduction. The simple fact is, that the current of the Thames, above the point at which the tides influence it, discharges in twenty-four hours eight hundred times this amount.

When the bill of this proposed water company was brought before the House of Commons for second reading, it became manifest that the imaginations of members were affected by such expressions as the "sources of the Thames," and "a million gallons daily," in much the same way as the imaginations of the ignorant. Though the quantity of water proposed to be taken bears, to the quantity which runs over Teddington weir, about the same ratio that a yard bears to half a mile, it was thought by many members that its loss would be a serious evil. No method of measurement would be accurate enough to detect the difference between the Thames as it now is, and the Thames *minus* the Cerney springs; and yet it was gravely stated in the House that, were the Thames diminished in the proposed way, "the proportion of sewage to pure water would be seriously increased." Taking a minute out of twelve hours, would be taking as large a proportion as the Cheltenham people wish to take from the Thames. Nevertheless, it was contended that to let Cheltenham have this quantity would be "to rob the towns along the banks of the Thames of their rights." Though, of the Thames flow-

ing by each of these towns, some 999 parts out of 1,000 pass by unused, it was held that a great injustice would be committed were one or two of these 999 parts appropriated by the inhabitants of a town who can now obtain daily but four gallons of foul water per head.

But the apparent inability thus shown to think of causes and effects in something like their true quantitive relations, was still more conspicuously shown. It was stated by several members that the Thames Navigation Commissioners would have opposed the bill if the commission had not been bankrupt; and this hypothetical opposition appeared to have weight. If we may trust the reports, the House of Commons listened with gravity to the assertion of one of its members, that, if the Cerney springs were diverted, "shoals and flats would be created." Not a laugh nor a cry of "Oh! oh," appears to have been produced by the prophecy, that the volume and scouring power of the Thames would be seriously affected by taking away from it twelve gallons per second! The whole quantity which these springs supply would be delivered by a current moving through a pipe one foot in diameter at the rate of less than two miles per hour. Yet, when it was said that the navigability of the Thames would be injuriously affected by this deduction, there were no shouts of derision. On the contrary, the House rejected the Cheltenham Water Bill by a majority of one hundred and eighteen to eighty-eight. It is true that the data were not presented in the above shape. But the remarkable fact is, that, even in the absence of a specific comparison, it should not have been at once seen that the water of springs, which drain but a few square miles at most, can be but an inappreciable part of the water which runs out of the Thames basin, extending over several thousand square miles. In itself, this is a matter of small moment. It interests us here simply as

an example of legislative judgment. The decision is one of those small holes through which a wide prospect may be seen, and a disheartening prospect it is. In a very simple case there is here displayed a scarcely credible inability to see how much effect will follow so much cause; and yet the business of the assembly exhibiting this inability is that of dealing with causes and effects of an extremely involved kind. All the processes going on in society arise from the concurrences and conflicts of human actions, which are determined in their nature and amounts by the human constitution as it now is—are as much results of natural causation as any other results, and equally imply definite quantitive relations between causes and effects. Every legislative act presupposes a diagnosis and a prognosis; both of them involving estimations of social forces and the work done by them. Before it can be remedied, an evil must be traced to its source in the motives and ideas of men as they are, living under the social conditions which exist—a problem requiring that the actions tending toward the result shall be identified, and that there shall be something like a true idea of the quantities of their effects as well as the qualities. A further estimation has then to be made of the kinds and degrees of influence that will be exerted by the additional factors which the proposed law will set in motion: what will be the resultants produced by the new forces coöperating with preëxisting forces—a problem still more complicated than the other.

We are quite prepared to hear the unhesitating reply, that men incapable of forming an approximately true judgment on a matter of simple physical causation may yet be very good law-makers. So obvious will this be thought by most, that a tacit implication to the contrary will seem to them absurd; and that it will seem to them absurd is one of the many indications of the profound ignorance that

prevails. It is true that mere empirical generalizations which men draw from their dealings with their fellows suffice to give them some ideas of the proximate effects which new enactments will work: and, seeing these, they think they see as far as needful. Discipline in physical science, however, would help to show them the utter inadequacy of calculating consequences based on simple data. And if there needs proof that calculations of consequences so based are inadequate, we have it in the enormous labor annually entailed on the Legislature in trying to undo the mischiefs it has previously done.

Should any say that it is useless to dwell on this incompetency, seeing that the House of Commons contains the select of the nation, than whose judgments no better are to be had, we reply, that there may be drawn two inferences which have important practical bearings. In the first place, we are shown how completely the boasted intellectual discipline of our upper classes fails to give them the power of following out in thought, with any correctness, the sequences of even simple phenomena, much less those of complex phenomena. And, in the second place, we may draw the corollary, that if the sequences of those complex phenomena which societies display, difficult beyond all others to deal with, are so unlikely to be understood by them, they may advantageously be restricted in their interferences with them.

In one direction, especially, shall we see reason to resist the extension of legislative action. There has of late been urged the proposal that the class contemptuously described as dividing its energeis between business and bethels shall have its education regulated by the class which might, with equal justice, be described as dividing its energies between club-rooms and game preserves. This scheme does not seem to us a hopeful one. Considering

that during the last half century our society has been remoulded by ideas that have come from the proposed pupil, and have had to overcome the dogged resistance of the proposed teacher, the propriety of the arrangement is not obvious. And if the propriety of the arrangement is not obvious on the face of it, still less obvious does it become when the competency of the proposed teacher comes to be measured. British intelligence, as distilled through the universities and redistilled into the House of Commons, is a product admitting of such great improvement in quality, that we should be sorry to see the present method of manufacture extended and permanently established.

VII.

POLITICAL FETICHISM.

[FROM THE READER FOR JUNE 10, 1865.]

POLITICAL FETICHISM.

A Hindoo, who, before beginning his day's work, salaams to a bit of plastic clay, out of which, in a few moments, he has extemporized a god in his own image, is an object of amazement to the European. We read with surprise bordering on scepticism of worship done by machinery, and of prayers which owe their supposed efficacy to the motion given by the wind to the papers they are written on. When told how certain of the Orientals, if displeased with their wooden deities, take them down and beat them, men laugh and wonder.

Why should men wonder? Kindred superstitions are exhibited by their fellows every day—superstitions that are, indeed, not so gross, but are intrinsically of the same nature. There is an idolatry which, instead of carving the object of its worship out of dead matter, takes humanity for its raw material, and expects, by moulding a mass of this humanity into a particular form, to give it powers or properties quite different from those it had before it was moulded. In the one case as in the other, the raw material is, as much as may be, disguised; there are decorative appliances by which the savage helps himself to think that he has something more than wood before him; and

the citizen gives to the political agencies he has helped to create, such imposing externals and distinctive names expressive of power as serve to strengthen his belief in the benefits prayed for. Some faint reflection of that "divinity" which "doth hedge a king" spreads down through every state department to the lowest rank, so that, in the eyes of the people, even the policeman puts on along with his uniform a certain indefinable power—nay, the mere dead symbols of authority excite reverence in spite of better knowledge; a legal form of words seems to have something especially binding in it, and there is a preternatural efficiency about a government-stamp.

The parallelism is still more conspicuous between the persistency of faith in the two cases, notwithstanding perpetual disappointments. It is difficult to perceive how graven images, that have been thrashed for not responding to their worshipper's desire, should still be reverenced and petitioned; but the difficulty of conceiving this is diminished when we remember how, in their turns, all the idols in our political pantheon undergo castigations for failing to do what was expected of them, and are nevertheless daily looked up to in the trustful hope that future prayers will be answered. The stupidity, the slowness, the perversity, the dishonesty of officialism, in one or other of its embodiments, are demonstrated afresh in almost every newspaper that issues. Probably half the leading articles written have for texts some absurd official blunder, some exasperating official delay, some astounding corruption, some gross official injustice, some incredible official extravagance. And yet these whippings, in which balked expectation continually vents itself, are immediately followed by renewed faith; the benefits that have not come are still hoped for, and prayers for others are put up. Along with proof that the old State machines are in themselves inert, and owe

such powers as they seem to have to the public opinion that sets their parts in motion, there are continually proposed new state machines of the same type as the old. This inexhaustible credulity is counted on by men of the widest political experience. Lord Palmerston, who probably knows his public better than any other man, lately said, in reply to a charge made in the House—"I am quite convinced that no person belonging to the government, in whatever department he may be, high or low, would be guilty of any breach of faith in regard to any matter confided to him." To assert as much in the face of facts continually disclosed, implies that Lord Palmerston knows well that men's faith in officialism survives all adverse evidence.

In which case are the hopes from state agency realized? One might have thought that the vital interests at stake would have kept the all-essential apparatus for administering justice up to its work; but they do not. On the one hand, here is a man wrongly convicted, and afterward proved to be innocent, who is "pardoned" for an offence he did not commit; and has this as consolation for his unmerited suffering. On the other hand, here is a man whose grave delinquencies a Lord Chancellor overlooks, on partial restitution being made—nay, more, countenances the granting of a pension to him. Proved guilt is rewarded, while proved innocence is left without compensation for pains borne and fortunes blasted! This marvellous antithesis, if not often fully paralleled in the doings of officialism as administrator of justice, is, in endless cases, paralleled in part. The fact that imprisonment is the sentence on a boy for stealing a pennyworth of fruit, while thousands of pounds may be transferred from a public into a private purse without any positive punishment being adjudged, is an anomaly kept in countenance by numerous

other judicial acts. Theoretically, the state is a protector of the rights of subjects; practically, the state continually plays the part of aggressor. Though it is a recognized principle of equity that he who makes a false charge shall pay the costs of the defence, yet, until quite recently, the Crown has persisted in refusing to pay the costs of citizens against whom it has brought false charges. Nay, worse, deliberate attempts used to be made to establish charges by corrupt means. Within the memory of those now living, the Crown, in excise-prosecutions, bribed juries; when the verdict was for the Crown, the custom was to give double fees; and the practice was not put an end to until the counsel for a defendant announced in open court that the jury should have double fees if their verdict was for his client!

Not alone in the superior parts of our judicial apparatus is this ill-working of officialism so thrust on men's notice as to have become proverbial; not alone in the life-long delays and ruinous expenses that have made Chancery a word of dread; not alone in the extravagances of bankruptcy courts, that lead creditors carefully to shun them; not alone in that uncertainty which makes men submit to gross injustice rather than risk the still grosser injustice which the law will, as likely as not, inflict on them; but down through the lower divisions of the judicial apparatus are all kinds of failures and absurdities daily displayed. It may be fairly urged in mitigation of the sarcasms current respecting the police, that among so many men cases of misconduct and inefficiency must be frequent; but we might have expected the orders under which they act to be just and well considered. Very little inquiry shows that they are not. There is a story current that, in the accounts of an Irish official, a small charge for a telegram, which an emergency had called for, was objected to at the head office in

London, and, after a long correspondence, finally allowed, but with the understanding that in future no such item would be passed, unless the department in London had authorized it! We cannot vouch for this story, but we can vouch for something which gives credibility to it. A friend who had been robbed by his cook went to the police-office, detailed the case, gave good reasons for inferring the direction of her flight, and requested the police to telegraph, that she might be intercepted. He was told, however, that they could not do this without authority; and this authority was not to be had without a long delay. The result was that the thief, who had gone to the town at the time supposed, escaped, and has not since been heard of. Take another function assumed by the police—the regulation of traffic. Daily, all through London, ten thousand fast-going vehicles, with hard-pressed men of business in them, are stopped by a sprinkle of slow-going carts and wagons. Greater speed in these comparatively few carts and wagons, or limitation of them to early and late hours, would immensely diminish the evil. But, instead of dealing with these really great hinderances to traffic, the police deal with that which is practically no hindrance. Men with advertisement-boards were lately forbidden to walk about, on the groundless plea that they are in the way; and incapables, prevented thus from getting a shilling a day, were driven into the ranks of paupers and thieves. Worse cases may be observed. For years past there has been a feud between the police and the orange girls, who are chased hither and thither because they are said to be obstructions to foot-passengers. Meanwhile, in some of the chief thoroughfares, may constantly be seen men standing with toys, which they delude children and their parents into buying by pretending that the toys make certain sounds, which they themselves make, and when the police, quietly

watching this obtainment of money under false pretences, are asked why they do not interfere, they reply that they have no orders. Admirable contrast! Trade dishonestly, and you may collect a small crowd on the pavement without complaint being made that you interrupt the traffic. Trade honestly, and you shall be driven from the pavement-edge as an impediment—shall be driven to dishonesty!

One might have thought that the notorious inefficiency of officialism as a protector against injustice would have made men skeptical of its efficiency in other things. If here, where citizens have such intense interests in getting a function well discharged, they have failed through all these many centuries in getting it well discharged—if this agency, which is in theory the guardian of each citizen, is in so many cases his enemy, that going to law is suggestive of impoverishment and possible ruin, it might have been supposed that officialism would scarcely be expected to work in all directions where the interests at stake are less intense. But so strong is the influence of political fetichism, that neither these experiences, nor the parallel experiences which every state-department affords, diminish men's faith. For years past there has been thrust before them the fact that, of the funds of Greenwich Hospital, one-third goes to maintain the sailors, while two-thirds go in administration; but this and other such facts do not stop their advocacy of more public administrations. The parable of straining at gnats and swallowing camels they see absolutely paralleled by officialism, in the red-tape particularity with which all minute details are enforced, and the astounding carelessness with which the accounts of a whole department, like the Patent Office, are left utterly uncontrolled; and yet we continue to hear men propose government-audits as checks for mercantile companies! No diminution of confidence seems to result from the disclos-

ure of stupidities which even a wild imagination would scarcely have thought possible; instance the method of promotion lately made public, under which a clerk in one branch of a department takes the higher duties of some deceased superior clerk, without any rise of salary, while some clerk in another branch of the department gets the rise of salary without any increase in his responsibilities!

Endless are these evils and absurdities, and surviving generation after generation, as they do, spite of commissions and reports and debates, there is an annual crop of new schemes for government agencies that are expected by citizens to work just as they propose them to work. With a system of army promotion which insures an organized incompetence, but which survives perpetual protests; with a notoriously ill-constituted admiralty, of which the doings are stock-subjects of ridicule; with a church that maintains its most effete formulas, notwithstanding almost universal repudiation of them; there are daily fresh demands for more law-established appliances. With building acts under which arise houses less stable than those of the last generation; with coal-mine inspection that does not prevent coal-mine explosions; with railway inspection that has for its accompaniment plenty of railway accidents —with these and other such failures continually displayed, there still prevails what M. Guizot rightly calls that "gross delusion, a belief in the sovereign power of political machinery."

A great service would be done by any man who would analyze the legislation, say of the last half century, and compare the expected results of Acts of Parliament with their proved results. He might make it an instructive revelation by simply taking all the preambles, and observing how many of the evils to be rectified were evils produced by preceding enactments. His chief difficulty would

be that of getting within any moderate compass the immense number of cases in which the benefits anticipated were not achieved, while unanticipated disasters were caused. And then he might effectively close his digest by showing what immense advantages have, in instance after instance, followed the entire cessation of legislative action; not, indeed, that such an accumulation of cases, however multitudinous and however conclusive, would have an appreciable effect on the average mind. Political fetichism will continue so long as men remain without scientific discipline—so long as they recognize only proximate causes, and never think of the remoter and more general causes by which their special agencies are set in motion. Until the thing which now usurps the name of education has been dethroned by a true education, having for its end to teach men the nature of the world they live in, new political delusions will grow up as fast as old ones are extinguished. But there is a select class existing, and a larger select class arising, on whom a work of the kind described would have an effect, and for whom it would be well worth while to write it.

VIII.

WHAT IS ELECTRICITY?

[FROM THE READER FOR NOVEMBER 19, 1864.]

WHAT IS ELECTRICITY?

PROBABLY few, if any, competent physicists have, of late years, used the term "electric fluid" in any other than a conventional sense. When distinguishing electricity into the two kinds, "positive" and "negative," or "vitreous" and "resinous," they have used the ideas suggested by these names merely as convenient symbols, and not as representatives of different entities. And, now that heat and light are proved to be modes of motion, it has become obvious that all the allied manifestations of force must be modes of motion.

What is the particular mode of motion which constitutes electricity, thus becomes the question. That it is some kind of molecular vibration, different from the molecular vibrations which luminous bodies give off, is, I presume, taken for granted by all who bring to the consideration of the matter a knowledge of recent discoveries. Beyond those simple oscillations of molecules, from which light and heat result, may we not suspect that there will, in some cases, arise compound oscillations? Let us consider whether the conditions under which electricity arises are not such as to generate compound oscillations; and whether the phenomena of electricity are not such as

must result from oscillations ; and whether the phenomena of electricity are not such as must result from compound oscillations.

The universal antecedent to the production of electricity is the immediate or mediate contact of heterogeneous substances—substances that are heterogeneous either in their molecular constitutions, or in their molecular states. If, then, electricity is some mode of molecular motion, and if, whenever it is produced, the contact of substances having unlike molecules, or molecules in unlike states, is the antecedent, there seems thrust upon us the conclusion that electricity results from some mutual action of molecules whose motions are unlike. What must this mutual action be ?

Before proceeding to answer this question, it will be needful to dispose of a demurrer that may be entered against the assumption, that unlike molecules have unlike motions in whatever states of aggregation they may be. It is currently admitted that, so long as they exist in the form of a gas, the particles of each kind of matter have a rate of vibration peculiar to themselves—a rate unlike the rates which the particles of other kinds of matter have. Prof. Tyndall has shown further that, when aggregated into a liquid, particles of any kind still maintain a rate of vibration synchronous with that which they had when diffused as a gas. But it is alleged that, on coalescing into solid masses, particles of different orders no longer maintain their distinctive rates of vibration. It is concluded that they severally take on vibrations of all orders, because solid matters, of whatever kinds, send off ethereal undulations of all lengths ; as is proved by the fact that each of them produces a continuous spectrum. I venture to think, however, that this inference is not a legitimate one. It seems to me demonstrably at variance with ultimate me-

chanical laws; and I think the facts are explicable without assuming it. To take the first—the *a priori* argument—it is incongruous with the doctrine of the persistence of force. Any difference between the vibrations of two orders of molecules, A and B, existing in a gaseous state, implies some kind of difference between the characters of the molecules. Be this a difference of inertia, of bulk, or of form, matters not to the argument; in any case, it is expressible as some unlikeness between the forces with which the molecules severally act and react on the medium that moves them. To say that, under the same conditions, the molecules A and B have different rates of vibration, though there exists between them no differential force, is to assert an effect without a cause, which is to deny the persistence of force. And if there exists between them some differential force, by virtue of which they react differently on incident forces, and acquire different rates of vibration, then this differential force must continue, under all states of aggregation, to produce its differential effect. To say that, when molecules of the kind A and molecules of the kind B are severally aggregated into solids, there ceases to be any distinction between their vibrations, is to say that the differential force ceases to produce any effect, and this is to deny the persistence of force. But now, passing to the *a posteriori* aspect of the question, it will be asked, How, then, can two solids, unlike in the natures of their molecules, severally produce, when heated, spectra that appear to be identical—spectra that severally imply ethereal undulations of all lengths? The answer to this question is to be sought in the effects produced on the mutual actions of molecules by their state of aggregation. Were all the particles similarly conditioned—were they all restrained by each other in like ways and degrees, then no reason for differences in their times of vibration could be

assigned. But they are differently conditioned in two ways—one of them contingent, the other necessary. In the first place, the process of consolidation, however it has gone on, is almost sure to have induced unlike states of tension throughout the mass—here the crystallization being more complete; there the cooling having gone on more rapidly. In the second place, the superficial particles, the layer of particles below it, and the subjacent particles to some depth, are subject to sets of restraining forces quite different from those which the inner particles are subject to; since, while the inner particles are exposed to the actions of particles all around them, the outer particles are exposed to such actions only on one side. And, as the periods of oscillation must be in part determined by the amounts and distributions of the tensions, it follows that the rates of oscillation of particles on the surface must be unlike those of particles near the surface, and progressively more unlike those of particles successively farther away from the surface. Hence, besides impressing on the surrounding medium undulations corresponding with their own, the surface-molecules will conduct to the surrounding medium the somewhat different undulations passed on to them by the subjacent molecules; and the still more different undulations passed on to them by molecules placed still deeper, and so on. Besides waves like their own, and waves a little unlike their own, and waves still more unlike their own, they will generate waves of various orders widely unlike their own. They will give off various vibrations shorter than their own, answering to the *differences* between the vibrations conveyed through them; and various vibrations longer than their own, answering to the periodic *coincidences* of the vibrations conveyed through them. Thus it becomes comprehensible how molecules of two different orders, having strongly contrasted rates of

vibration, may, when severally aggregated with solid masses, both produce continuous spectra, and so appear to be in like states of agitation.

From this preliminary explanation, let us now return to the question propounded—What must be that mutual action of molecules having unlike motions, which, as we see, is the universal antecedent of electrical disturbance? The answer to this question does not seem difficult to reach, if we take the simplest case—the case of contact-electricity. When two pieces of metal of the same kind, and at the same temperature, are applied to one another, there is no electrical excitation; but, if the metals applied to one another be of different kinds, there is a genesis of electricity. This, which has been regarded as an anomalous fact—a fact so anomalous that it has been much disputed because apparently at variance with every hypothesis—is a fact to which an interpretation is at once supplied by the hypothesis that electricity results from the mutual disturbances of unlike molecular motions. For, if, on the one hand, we have homogeneous metals in contact, their respective molecules, oscillating synchronously, will give and take any forces which they impress on one another without producing an oscillation of a new order. But, if, on the other hand, the molecules of the one mass have periods of oscillation different from those of the other mass, then their mutual impacts will not agree with the period of oscillation of either, but will generate a new rhythm, differing from, and much slower than, that of either. The production of what are called "beats" in acoustics, will best illustrate this. It is a familiar fact that two strings, vibrating at different rates, from time to time concur in sending off aërial waves in the same direction at the same instant; that then, their vibrations getting more and more out of correspondence, they send off their aërial waves in

the same direction at exactly intermediate instants; and presently, coming once more into correspondence, they again generate coinciding waves. So that, when their periods of vibration differ but little, and when consequently it takes an appreciable time to complete their alternations of agreement and disagreement, there results an audible alternation in the sound—a succession of pulses of louder and feebler sound. In other words, besides the primary, simple, and rapid series of waves, constituting the two sounds themselves, there is a series of slow compound waves, resulting from their repeated conflicts and concurrences. Now, if, instead of the two strings communicating their vibrations to the air, each communicated its vibrations to the other, we should have just the same alternation of concurrent and conflicting pulses. And if each of the two strings was combined with an aggregate of others like itself, in such way that it communicated to its neighbors both its normal and its abnormal vibrations, it is clear that through each aggregate of strings there would be propagated one of these compound waves of oscillation, in addition to their simple rapid oscillations. This illustration will, I think, make it manifest that when a mass of molecules, which have a certain period of vibration, is placed in contact with a mass of molecules which have another period of vibration, there must result an alternation of coincidences and antagonisms in the molecular motions, such as will make the molecules alternately increase and decrease one another's motions. There will be instants at which they are moving in the same direction, and intervening instants at which they are moving in opposite directions; whence will arise periods of greatest and least deviations from their ordinary motions. And these greatest and least deviations, being communicated to neighboring molecules, and passed on by them to the next, will

result in waves of perturbation propagated throughout each mass.

Let us now ask what will be the mutual relations of these waves. Action and reaction being equal and opposite, it must happen that whatever effect a molecule of the mass A produces upon an adjacent molecule of the mass B, must be accompanied by an equivalent reverse effect upon itself. If a molecule of the mass A is at any instant moving in such way as to impress on a molecule of the mass B an additional momentum in any given direction, then the momentum of the molecule of B, in that direction, will be diminished to an equal amount. That is to say, to any wave of increased motion propagated through the molecules of B, there must be a reactive wave of decreased motion propagated in the opposite direction through the molecules of A. See, then, the two significant facts. Any *addition* of motion, which at one of these alternate periods is given by the molecules of A to the molecules of B, must be propagated through the molecules of B in a direction *away from* A; and simultaneously there must be a *subtraction* from the motion of the molecules of A, which will be propagated through them in a direction *away from* B. To every wave of *excess* sent through the one mass, there will be a corresponding wave of *defect* sent through the other; and these *positive* and *negative* waves will be exactly coincident in their times, and exactly equal in their amounts. Whence it obviously follows that, if these waves, proceeding from the surface of contact through the two masses in contrary directions, are brought into relation, they will neutralize each other. Action and reaction being equal and opposite, these *plus* and *minus* molecular motions will cancel one another if they are added together, and there will be a restoration of equilibrium.

These positive and negative waves of perturbation will

travel through the two masses of molecules with great facility. It is now an established truth that molecules absorb, in the increase of their own vibrations, those rhythmical impulses or waves which have periodic times the same as their own; but that they cannot thus absorb successive impulses that have periodic times different from their own. Hence these differential undulations, being very long undulations in comparison with those of the molecules themselves, will readily pass through the masses of molecules, or be *conducted* by them. Further observe that, if the two masses of molecules continue joined, these positive and negative differential waves travelling away from the surface of contact in opposite directions, and severally arriving at the outer surfaces of the two masses, will be reflected from these; and, travelling back again toward the surface of contact, will there meet and neutralize one another. Hence no current will be produced along a wire joining the outer surfaces of the masses; since neutralization will be more readily effected by this return of the waves through the masses themselves. But, though no external current arises, the masses will continue in what we call opposite electric states; as a delicate electrometer shows that they do. And further, if they are parted, the positive and negative waves which have the instant before been propagated through them respectively, remaining unneutralized, the masses will display their opposite electric states in a more conspicuous way. The residual positive and negative waves will then neutralize each other along any conductor that is placed between them, seeing that the *plus* waves communicated from the one mass to the conductor, meeting with the *minus* waves communicated from the other, and being mutually cancelled as they meet, the conductor will become a line of least resistance to the waves of each mass.

Let us pass now to the allied phenomena of thermo-electricity. Suppose these two masses of metal to be heated at their surfaces of contact; the forms of the masses being such that their surfaces of contact can be considerably heated without their remoter parts being much heated. What will happen? Prof. Tyndall has shown, in the cases of various gases and liquids, that, other things equal, when molecules have given to them more of the insensible motion which we call heat, there is no alteration in their periods of oscillation, but an increase in the dimensions of their oscillations; the molecules make wider excursions in the same times. As above implied, we have good reason to conclude that the like is true of solids; the apparent proof of changed periods of vibration being explicable in the manner shown. Assuming this, it will follow that, when the two metals are heated at their surfaces of contact, the result will be the same as before in respect of the natures and intervals of the differential waves. There will be a change, however, in the strengths of these waves. For, if the two orders of molecules have severally given to them increased quantities of motion, the perturbations which they impress on each other will also be increased. These somewhat stronger positive and negative waves of differential motion will, as before, travel through either mass away from the surfaces of contact—that is, toward the cold extremities of the masses. From these cold extremities they will, as before, rebound toward the surfaces of contact; and as before will tend thus to equilibriate each other. But they will meet with resistance in thus travelling back. It is a well-ascertained fact that raising the temperatures of metals decreases their conducting powers. Hence, if the two cold ends of the masses be put in connection by some other mass whose molecules can take on with facility these differential undulations—

that is, if the two ends be joined by a conductor, the positive and negative waves will meet and neutralize one another along this conductor, instead of being reflected back to the surfaces of contact. In other words, there will be established a current along the wire joining the two cold ends of the metallic masses.

Carried a step further, this reasoning affords us an explanation of the thermo-electric pile. If a number of these bars of different metals, as antimony and bismuth, are soldered together, end to end, in alternate order, AB, AB, AB, etc., then, so long as they remain cold, there is no manifestation of an electric current; or, if all the joints are equally heated, there is no manifestation of an electric current beyond that which would arise from any relative coolness of the two ends of the compound bar. But, if alternate joints are heated, an electric current is produced in a wire joining the two ends of the compound bar—a current that is intense in proportion to the number of pairs. What is the cause of this? Clearly, so long as all the joints are of the same temperature, the differential waves propagated from each joint toward the two adjacent joints will be equal and opposite to those from the adjacent joints, and no disturbance will be shown. But if alternate joints are heated, the positive and negative differential waves propagated away from them will be stronger than those propagated from the other joints. Hence, if the joint of bar A with bar B be heated, the other end of the bar B, which is joined to A2, not being heated, will receive a stronger differential wave than it sends back. In addition to the wave which its molecules would otherwise induce in the molecules of A2, there is an effect which it conducts from A1; and this extra impulse propagated to the other end of B2 is added to the impulse which its heated molecules would otherwise give to the molecules of A3; and

so on throughout the series. The waves being added together, become more violent, and the current through the wire joining the extremities of the series, more intense.

This interpretation of the facts of thermo-electricity will probably be met by the objection that there are, in some cases, thermo-electric currents developed between masses of metal of the same kind, and even between different parts of the same mass. It may be urged that, if unlikeness between the rates of vibration of molecules in contact is the cause of these electric disturbances; then, heat ought not to produce any electric disturbances when the molecules are of the same kind; since we have reason to conclude that heat does not change the periodic times of molecular vibrations. This objection, which seems at first sight a serious one, introduces us to a confirmation. For, where the masses of molecules are homogeneous in all other respects, difference of temperature does *not* generate any thermo-electric current. The junction of hot with cold mercury sets up no electric excitement. In all cases where thermo-electricity is generated between metals of the same kind, there is evidence of heterogeneity in their molecular structures—either one has been hammered and the other not, or one is annealed and the other unannealed. And, where the current is between different parts of the same mass, there are differences in the crystalline states of the parts, or differences between the ways in which the parts have cooled after being cast. That is to say, there is proof that the molecules in the two masses, or in different parts of the same mass, are in unlike relations to their neighbors—are in unlike states of tension. Now, however true it may be that molecules of the same kind vibrate at the same rate, whatever may be their temperature, it is obviously true so long only as their motions are not modified by restraining forces. If molecules of the same kind

are in one mass arranged into that state which produces crystallization, while in another mass they are not thus bound together; or if in the one their molecular relations have been modified by hammering, and in the other not; the differences in the restraints under which they respectively vibrate will effect their rates of vibration. And if their rates of vibration are rendered unequal, then the alleged cause of electrical disturbance comes into existence.

To sum up, may it not be said that by some such action alone can the phenomena of electricity be explained; and that some such action must inevitably arise under the conditions? On the one hand, electricity, being a mode of motion, implies the transformation of some preëxisting motion—implies also, a transformation such that there are two new kinds of motion simultaneously generated, equal and opposite in their directions—implies further that these differ in being *plus* and *minus*, and being therefore capable of neutralizing each other. On the other hand, in the above cases, molecular motion is the only source of motion that can be assigned; and this molecular motion must, under the circumstances, produce effects of the kind witnessed. Molecules vibrating at different rates cannot be brought in proximity without affecting one another's motions. They must affect one another's motions by periodically adding to, or deducting from one another's motions; and any excess of motion which those of the one order receive, must be accompanied by an equivalent defect of motion in those of the other order. When such molecules are units of aggregates placed in contact, they must pass on these pertubations to their neighbors. And so, from the surface of contact, there must be waves of excessive and defective molecular motion, equal in their amounts, and opposite in their directions—waves which must exactly compensate

one another when brought into relation. In brief, I think it will be admitted that the cause alleged is "a true cause," and that it is a cause calculated to work some such effects as those described.

I have here dealt only with electrical phenomena of the simplest kind. Hereafter I may possibly endeavour to show how this hypothesis furnishes interpretations of other forms of Electricity.

POSTSCRIPT.—During the nine years that have elapsed since the foregoing essay was published, I have never found myself any nearer to such allied interpretations of other forms of Electricity. Though, from time to time, I have recurred to the subject, in the hope of fulfilling the expectation raised by the closing sentence, yet no clue has encouraged me to pursue the speculation. Only now, when republication of the essay in a permanent form once more brings the question before me, does there occur a thought which appears worth setting down.

The union of two different ideas, not before placed in juxtaposition, has generated this thought. In the first number of the *Principles of Biology*, issued in January 1863, and dealing, among other "Data of Biology," with organic matter and the effects of forces upon it, I ventured to speculate about the molecular actions concerned in organic changes, and, among others, those by which light enables plants to assimilate the carbon from carbonic acid (§ 13.) Pointing out that the ability of heat to decompose compound molecules, is generally proportionate to the difference between the atomic weights of their component elements—assuming that components having widely-unlike atomic weights, have widely-unlike motions, and are therefore affected by widely-unlike undulations; the inference drawn was, that

in proportion as the rhythms of its components differ, a compound molecule will be unstable in presence of strong etherial undulations acting upon one component more than on the other or others: their movements thus being rendered so incongruous that they can no longer hold together. It was argued, further, that a tolerably-stable compound molecule may, if exposed to strong etherial undulations especially disturbing one of its components, be decomposed when in presence of some unlike molecule having components whose times of oscillation differ less from those of this disturbed component. And a parallel was drawn between the de-oxidation of metals by carbon when exposed to the longer undulations in a furnace, and the de-carbonization of carbonic acid by hydrogen, &c., when exposed to the shorter undulations in a plant's leaves. These ideas I recall chiefly for the purpose of presenting clearly the conception of a compound molecule as containing diversely-moving components—components having independent and unlike oscillations, in addition to the oscillation of the whole molecule formed by them. The legitimacy of this conception may, I suppose, be assumed. The beautiful experiments by which Prof. Tyndall has proved that light decomposes the vapours of certain compounds, illustrates this ability which the elements of a compound molecule have, severally to take up etherial undulations corresponding to their own; and thus to have their individual movements so increased as to cause disruption of the compound molecule. This, at least, is the interpretation which Prof. Tyndall puts on the facts; and I presume that he puts a kindred interpretation upon the facts he has disclosed respecting the marvellous power possessed by complex-moleculed vapours to absorb heat—the interpretation, namely, that the thermal undulations are, in such vapours, taken up in augmenting the move-

ments within each molecule, rather than in augmenting the movements of the molecules as wholes.

But now, assuming this to be a true conception of compound molecules and the effects produced on them by etherial undulations, there presents itself the question—What will be the effects produced by compound molecules on one another? How will the elements of one compound molecule have their rhythmical motions affected by proximity to the elements of an unlike compound molecule? May we not suspect that effects will be produced on one another, not only by the unlike molecules as wholes, but also certain other, and partially-independent, effects by their components on one another; and that there will so be generated some specialized form of molecular motion? Throughout the speculation set forth in the foregoing essay, the supposition is that the molecules are those of juxtaposed metals—molecules which, whether absolutely simple or not, are relatively simple; and these are regarded as producing on one another's movements perturbations of a relatively-simple kind, that admit of being transferred from molecule to molecule throughout each mass. In trying to carry further this interpretation, it had not occurred to me until now, to consider the perturbations produced on one another by compound molecules: taking into consideration, not merely the capacity each has for affecting the other as a whole, but the capacity which the constituents of each individually have for affecting the individual constituents of the other. If an individual constituent of a compound molecule can, by the successive impacts of etherial undulations, have the amplitudes of its oscillations so increased as to detach it; we can scarcely doubt that an individual constituent of a compound molecule may affect an individual constituent of an unlike

compound molecule near it: their respective oscillations perturbing one another apart from the perturbation produced on one another by the compound molecules as wholes. And it seems inferable that the secondary perturbation thus arising, will, like the primary perturbation, be such that the action and reaction, equal and opposite in their amounts, will produce equal and opposite deviations in the molecular movements. From this there appear to be several corollaries.

If a compound molecule, having a slow rhythm as a whole in addition to the more rapid rhythms of its members, has the power of taking up much of that motion we call heat in the increase of its internal movements, and to a corresponding degree takes up less in the increase of its movements as a whole; then may we not infer that the like will hold when other kinds of forces are brought to bear on it? May we not anticipate that when a mass of compound molecules of one kind is made to act upon a mass of compound molecules of another kind (say by friction), the molecular effects mutually produced, partly in agitating the molecules as wholes, and partly in agitating their components relatively to one another, will become less of the first and more of the last, in proportion as the molecules progress in compositeness?

A further implication suggests itself. While much of the force mutually exercised will thus go to increase the motion within each of the compound molecules that immediately act on one another, it appears inferable that relatively little of this intestinal motion will be communicated to other molecules. The excess of oscillation given to individual members of a large cluster, will not be readily passed on to homologous members of adjacent large clusters; since they must be relatively far apart. Whatever motion is transferred, must be transferred by

waves of the intervening etherial medium; and the power of these must decrease rapidly as the distance increases. Obviously such difficulty of transfer must, for this reason, become great when the molecules become highly compounded.

At the same time will it not follow that such augmentations of movement caused in individual members of a cluster, not being readily transmissible to homologous members of adjacent clusters, will accumulate; so that the more composite molecules become, the more possible will it be for individual components of them to be violently affected by individual components of different composite molecules near them—the more possible will it be for the mutual perturbations of such to accumulate?

And now let us consider how these inferences bear on the interpretation of Statical Electricity—the form of Electricity most unlike the form above dealt with.

The substances which exhibit most conspicuously the phenomena of statical electricity are distinguished by the chemical complexity of their molecules, or else by the compositeness of their molecules produced allotropically or isomerically, or else by both. The simple substances electrically excited by friction, as carbon and sulphur, are those having several allotropic states—those capable of forming multiple molecules. The conchoidal fracture of the diamond and of roll-sulphur, suggest some colloidal form of aggregation, regarded by Prof. Graham as a form in which the molecules are united into relatively-large groups.* In such compound inorganic sub-

* Though conchoidal fracture may not be conclusive proof of colloidality, yet colloidal substances hard enough for fracture always display it. Respecting roll-sulphur I may say that though in a few days after it is made, it changes from its original crystalline state to a state in which it consists of minute crystals of another kind

stances as glass, we have, besides the chemical complexity, this same conchoidal fracture which, along with other evidence, shows glass to be a colloid; and the colloidal form of molecule is to be similarly inferred as characterizing resin, amber, &c. That dry animal substances, such as silk and hair, are formed of extremely-large molecules, we have clear proof; since these, chemically complex in a high degree, also have their components united in high multiples. It needs but to name the fact that non-electric and conducting substances, such as the metals, acids, water, &c., have relatively-simple molecules, to make it clear that the capacity for developing statical electricity depends in some way upon the presence of molecules of highly-composite kinds. And there is even still more conclusive proof than that yielded by the contrast between these groups—the proof furnished by the fact that the same substance may be a conductor or a non-conductor, according to its form of molecular aggregation. Thus selenium when crystalline is a conductor, but when in that allotropic state called amorphous, or non-crystalline, it is a good non-conductor. That is, accepting Prof. Graham's interpretation of these states, when its molecules are arranged singly, it is a conductor, but when compounded into groups it is a non-conductor, and, by implication, an electric.

So far, then, the *à priori* inference that a peculiar form of molecular perturbation will result when two unlike substances, one of which or each of which consists of highly-compounded molecules, are made to act on one

irregularly massed, yet there is reason for suspecting that these have a matrix of amorphous sulphur. I learn from Dr. Frankland that, when sublimed, sulphur aggregates partly into minute crystals and partly into an amorphous powder distinguished by insolubility.

another, is justified *à posteriori*. And now, instead of asking generally what will happen, let us ask what may be inferred to happen in a special case. A piece of glass is rubbed by silk. The large colloidal molecules forming the surface of each, are made to disturb one another. This is an inference about which there will, I suppose, be no dispute; since it is that assumed in the now-established doctrine of the correlation of heat and motion. Besides the effect which as wholes they mutually produce, there is the effect produced on one another by certain of their components. Such of these as have times of oscillation which differ, but not very widely, generate mutual perturbations that are equal and opposite. Could these perturbations be readily propagated away from the surface of contact through either mass, the effect would quickly dissipate, as in the case of metals; but, for the reason given above, these perturbations cannot be transferred with ease to the homologous members of the compound molecules behind. Hence the mechanical force of the friction, transformed into the molecular movements of these superficial constituent molecules, exists in them as *intense* mutual perturbations, which, unable to diffuse, are limited to the surfaces, and, indeed, to those parts of the surfaces that have acted on one another. In other words, the two surfaces become charged with two equal and opposite molecular perturbations—perturbations which, cancelling one another if the surfaces are kept in contact, cannot do this if the surfaces are parted; but can then cancel one another only if a conductor is interposed.

Let me briefly point out some apparent agreements between the corollaries from this hypothesis, and the observed phenomena.

We have, first, an interpretation of the fact, otherwise

seeming so anomalous, that this form of electrical excitement is *superficial.* That there should be a mode of activity limited to the surface of a substance, is difficult to understand in the absence of some conception of the kind suggested.

We have an explanation of the truth, insisted on by Faraday, that there can be no charge of one kind of electricity obtained, without a corresponding charge of the opposite kind. For it is a necessary implication of the hypothesis above set forth, that no molecular perturbation of the nature described, can be produced, without there being simultaneously produced a counter-perturbation exactly equal to it.

May we not also say that some insight is afforded into the phenomena of induction? In the cases thus far considered, the two surfaces electrified by the mutual perturbation of their molecules, are supposed to be in contact. Since, however, apparent contact is not actual contact, we must, even in this case, assume that the mutual perturbation is effected through an intervening stratum of ether. To interpret induction, then, we have first to conceive this stratum of ether to be greatly increased in thickness; and then to ask what will happen if the molecules of one surface, in this state of extreme internal perturbation, act on the molecules of a surface near it. Whether the stratum of ether is so thin as to be inappreciable to our senses, or whether it is wide enough to be conspicuous, it must still happen that if through it the mutual perturbations are conveyed in the one case, they will be conveyed in the other; and hence a surface which is already the seat of these molecular perturbations of one order, will induce perturbations of a counter order in the molecules of an adjacent surface.

In additional justification of the hypothesis, I will only

point out that voltaic electricity seems to admit of a kindred interpretation. For any molecular re-arrangement, such as occurs in a chemical decomposition and recombination, implies that the movements of the molecules concerned are mutually perturbed; and their perturbations must conform to the general law already described: the molecules must derange one another's motions in equal and opposite ways, and so must generate *plus* and *minus* derangements that cancel when brought into relation.

Of course I suggest this view simply as one occurring to an outsider. Unquestionably it presents difficulties; as, for instance, that no manifest explanation is yielded by it of electric attractions and repulsions. And there are doubtless objections not obvious to me that will at once strike those to whom the facts are more familiar. The hypothesis must be regarded as speculative; and as set down on the chance that it may be worth consideration.

Since the foregoing postscript was put in type, I have received criticisms upon it, oral and written, from several leading electricians and physicists; and I have profited by them to amend parts of the exposition. While I have remained without endorsements of the hypothesis, the objections raised have not been such as to make clear its untenability.

On one point an addition seems needful to exclude a misconstruction apt to arise. The description of the mutually-produced molecular perturbations, opposite in their kinds, as resulting in waves that are propagated away from the place of disturbance, and cancel when brought into relation, is met by the criticism, that waves, proceeding in opposite directions and meeting, do not

mutually cancel, but, passing one another, proceed onwards. There are, however, two respects in which the parallelism does not hold, between the waves referred to and the waves I have described, which perhaps cannot rightly be called waves. The waves referred to, as those on the surface of a liquid, are such that each consists of two opposite deviations from a mean state. Each shows excess and defect. A series of them is a series of *plus* and *minus* divergences; and if two such series meet one another, they do not cancel. But there is no analogy between this case and a case in which the whole effect propagated in one direction is a *plus* motion, and the whole effect propagated in the opposite direction is a *minus* motion—that is, *plus* and *minus* changes in other motions. These, if equal in amount, will cancel when they meet. If one is a continual addition to motion in a certain direction, and the other a corresponding subtraction from motion in that direction, the two, when added together, must produce zero. From another point of view the absence of parallelism between the two cases may be equally well seen. Waves of the kinds instanced as not cancelling one another, are waves produced by some force foreign to the medium exhibiting them—an extrinsic force. Hence, proceeding from the place of initiation, they are necessarily, considered in their totalities, *positive* in whatever directions they travel; and hence, too, when conducted round so as to meet, an exaggerated perturbation will result. But in the simplest of the cases here dealt with (that of contact-electricity) the perturbation is not of extrinsic origin, but of intrinsic origin. There is no external activity at the expense of which the quantity of motion in the disturbed matter is positively increased. The activity, being such only as is internally possessed, can generate no more motion than already

exists; and therefore whatever gain of motion arises anywhere in the molecules must be at the cost of an equal loss elsewhere. Here perturbation cannot be a *plus* motion in all directions from the place of initiation; but any *plus* motion continually generated can result only from an equal and opposite *minus* motion continually generated; and the mutual cancelling becomes a corollary from the mutual genesis.

In the course of the discussions which I have had, the following way of presenting the argument has occurred to me.

1. Two homogeneous bodies are rubbed together and there results heat: the interpretation being that the molar motion is transformed into molecular motion. Here motion produces motion—the *form* only being changed.

2. Now of the two bodies one is replaced by a body unlike in nature to the other, and they are again rubbed. Again a certain amount of heat is produced: some of the molar motion is, as before, transformed into molecular motion. But, at the same time, another part of the molar motion is changed into—what? Surely not a fluid, a substance, a thing. It cannot be that what in the first case produces a change of *state*, in the second case produces an *entity*. And in the second case itself, it cannot be that while part of the original motion becomes changed into another species of motion, part of it becomes changed into a species of matter.

3. Must we not say, then, that if, when the two bodies rubbed are homogeneous, sensible motion is transformed into insensible motion, when they are heterogeneous, sensible motion must still be transformed into insensible motion: such difference of nature as this insensible motion has, being consequent on the difference of nature

between the two kinds of molecules acting on one another?

4. If, when the two masses are homogeneous, those molecules which compose the two rubbed surfaces disturb one another, and increase one another's oscillations; then, when the two masses are heterogeneous, those molecules forming the two rubbed surfaces must also disturb one another in some way—increase one another's agitations.

5. If, when the two sets of molecules are alike in kind, the mutual disturbance is such that they simply increase the amplitudes of one another's oscillations, and do this because their times correspond; then, must it not be that when they are unlike in kind, the mutual disturbance will involve a differential action consequent on the unlikeness of their motions? Must not the discord of the oscillations produce a result which cannot be produced when the oscillations are concordant—a compound form of molecular motion?

6. If masses of relatively-simple molecules, placed in apposition and made to act on one another, cause such effects; then must we not say that effects of the same class, but of a different order, will be caused by the mutual actions, not of the molecules as wholes, but of their constituents? If the rubbed surfaces severally consist of highly-compounded molecules—each containing, it may be, several hundreds of minor molecules, united into a definitely-arranged cluster; then, while the molecules as wholes affect one another's motions, must we not infer that the constituents of the one class will affect the constituents of the other class in their motions? While molecules as wholes increase one another's oscillations, or derange one another's oscillations, or both, the components of them cannot be so stably arranged that members of

the one group are wholly inoperative on members of the other group. And if they are operative, then there must be a compound form of molecular motion which arises when masses of highly-compounded molecules of unlike kinds, are made to act on one another.

With this series of propositions and questions, I leave the suggestion to its fate; merely remarking that, setting out with the principles of molecular physics now accepted, it seems difficult to avoid the implication that some actions of the kinds described take place, and that there result from them some classes of phenomena—phenomena which, if not those we call electrical, remain to be identified.

IX.

THE CONSTITUTION OF THE SUN.

[FROM THE READER FOR FEBRUARY 25, 1865.]

THE CONSTITUTION OF THE SUN.

The hypothesis of M. Faye, which you have described in your numbers for January 28 and February 4, is to a considerable extent coincident with one which I ventured to suggest in an article on "Recent Astronomy and the Nebular Hypothesis," published in the "Westminster Review" for July, 1858. In considering the possible causes of the immense differences of specific gravity among the planets, I was led to question the validity of the tacit assumption that each planet consists of solid or liquid matter from centre to surface. It seemed to me that any other internal structure, which was mechanically stable, might be assumed with equal legitimacy. And the hypothesis of a solid or liquid shell, having its cavity filled with gaseous matter at high pressure and temperature, was one which seemed worth considering, since it promised an explanation of the anomalies named, as well as sundry others.

Hence arose the inquiry—What structure will result from the process of nebular condensation? "Starting with a rotating spheroid of aëriform matter, in the latter stages of its concentration, but before it has begun to take a liquid or solid form," it was argued that the actions going

on in it will be these—increasing aggregation, and consequent evolution of heat, which must be greater at the centre than at the surface, resulting want of equilibrium, and the setting up of a circulation of gases from the hottest part to the coolest part, along lines of least resistance to expansion: and hence an establishment of constant currents from the centre along the axis of rotation toward each pole followed by a flowing over of the accumulation at each pole in currents along the surface to the equator; such currents being balanced by the continual collapse, toward the centre, of gaseous matter lying in the equatorial plane. It was further argued that gases travelling from the centre by way of the poles to the equator, must be cooled first by expansion on approaching the surface, and afterward by freedom of radiation into space; and it was hence inferred that the outside of the spheroid at the equator will be the place of greatest refrigeration. It was concluded that the earliest precipitation will therefore occur in that region.

"An equatorial belt of vapor will be the first formed, and, widening into a zone, will by-and-by condense into a fluid (liquid). Gradually this fluid (liquid) film will extend itself on each side the equator, and, encroaching upon the two hemispheres, will eventually close over at the poles: thus forming a thin, hollow globe, or rather spheroid, filled with gaseous matter. We do not mean that this condensation will take place at the very outermost surface; for probably round the denser gases forming the principal mass there will extend strata of gases too rare to be entangled in these processes. It is the surface of this inner spheroid of denser gases to which our reasoning points as the place of earliest condensation."

"The internal circulation we have described continuing, as it must, after the formation of this liquid film, there

will still go on the radiation of heat, and the progressive aggregation. The film will thicken at the expense of the internal gaseous substances precipitated upon it. As it thickens, as the globe contracts, and as the gravitative force augments, the pressure will increase, and the evolution and radiation of heat will go on more rapidly. Eventually, however, when the liquid shall become very thick, and the internal cavity relatively small, the obstacle put to the escape of heat by this thick liquid, with its slowly-circulating currents, will turn the scale; the temperature of the outer surface will begin to diminish, and a solid crust will form while the internal cavity is yet unobliterated" (pp. 215, 216).

Omitting the various confirmations which this *a priori* conclusion was shown to derive from the contrasted specific gravities of the planets, as well as from sundry other peculiarities they present, I will pass to the deductions respecting the constitution of the sun which were drawn from this hypothesis. The process of condensation being in its essentials the same for all concentrating nebular spheroids, planetary or solar, it was argued that the sun is still passing through that incandescent stage which all the planets have long ago passed through: his later aggregation, joined with the immensely greater ratio of his mass to his surface, involving the comparative lateness of cooling. Supposing the sun to have reached the state of a molten shell, enclosing a gaseous nucleus, it was concluded that this molten shell, ever radiating its heat, but ever acquiring fresh heat by further integration of the sun's mass, will be constantly kept up to that temperature at which its substance evaporates.

"If we consider what must have been the state of things here when the surface of the earth was molten, we shall see that, round the still molten surface of the sun,

there probably exists a stratum of dense aëriform matter, made up of sublimed metals and metallic compounds, and above this a stratum of comparative rare medium analogous to air. What now will happen with these two strata? Did they both consist of permanent gases, they could not remain separate: according to a well-known law, they would eventually form a homogeneous mixture. But this will by no means happen when the lower stratum consists of matters that are gaseous only at excessively high temperatures. Given off from a molten surface, ascending, expanding, and cooling, these will presently reach a limit of elevation above which they cannot exist as vapor, but must condense and precipitate. Meanwhile, the upper stratum, habitually charged with its quantum of these denser matters, as our air with its quantum of water, and ready to deposit them on any depression of temperature, must be habitually unable to take up any more of the lower stratum; and therefore this lower stratum will remain quite distinct from it. We conclude, then, that there will be two concentric atmospheres, having a definite limit or separation" (pp. 222, 223).

To a revised edition of this essay, republished along with others in November, 1863, I made the following additions:

"Since the foregoing paragraph was originally published, in 1858, the proposition it annunciates as a corollary from the nebular hypothesis has been in great part verified. The marvellous disclosures made by spectrum analysis have proved beyond the possibility of doubt that the solar atmosphere contains, in a gaseous state, the metals iron, calcium, magnesium, sodium, chromium, and nickel, along with small quantities of barium, copper, and zinc. .... And here let us not omit to note also the significant bearing which Kirchhoff's results have on the doctrine

contended for in a foregoing section. Leaving out the barium, copper, and zinc, of which the quantities are inferred to be small, the metals existing as vapors in the sun's atmosphere, and by consequence as molten in his incandescent body, have an average specific gravity of 4.25. But the average specific gravity of the sun is about 1. How is this discrepancy to be explained? To say that the sun consists almost wholly of the three lighter metals named, would be quite unwarranted by the evidence: the results of spectrum analysis would just as much warrant the assertion that the sun consists almost wholly of the three heavier. Three metals (two of them heavy) having been already left out of the estimate because their quantities appear to be small, the only legitimate assumption on which to base an estimate of specific gravity, is that the rest are present in something like equal amounts. Is it, then, that the lighter metals exist in larger proportions in the molten mass, though not in the atmosphere? This is very unlikely; the known habitudes of matter rather imply that the reverse is the case. Is it, then, that, under the conditions of temperature and gravitation existing in the sun, the state of liquid aggregation is wholly unlike that existing here? This is a very strong assumption; it is one for which our terrestrial experience affords no adequate warrant; and, if such unlikeness exists, it is very improbable that it should produce so immense a contrast in specific gravity as that of 4 to 1. The more legitimate conclusion is that the sun's body is not made up of molten matter all through, but that it consists of a molten shell with a gaseous nucleus. And this we have seen to be a corollary from the nebular hypothesis." The conception of the sun's constitution thus set forth is like that of M. Faye in so far as the successive changes, the resulting structures, and the ultimate state are concerned; but unlike it

in so far as the sun is supposed to have reached a later stage of concentration. As I gather from your abstract of M. Faye's paper, he considers the sun to be at present a gaseous spheroid, having an envelope of metallic matters precipitated in the shape of luminous clouds, the local dispersions of which, caused by currents from within, appear to us as spots; and he looks forward to the future formation of a liquid film as an event that will rapidly be followed by extinction. Whereas the above hypothesis is that the liquid film already exists beneath the visible photosphere, and that extinction cannot result until, in the course of further aggregation, the gaseous nucleus has become so much reduced, and the shell so much thickened, that the escape of the heat generated is greatly retarded. I think this view escapes some objections to which that of M. Faye is open, and that it harmonizes with the appearances as well, if not better. Let us contrast the two.

Though the specific gravity of the sun is so low as almost to negative the supposition that its body consists of solid or liquid matter from center to surface, yet it seems higher than is probable for a gaseous spheroid with a cloudy envelope. Possibly, notwithstanding intense temperature, the gravitation of the sun's substance toward its centre might be great enough to produce considerable density in its interior; but that the interior density of a gaseous medium might be thus made great enough to give the entire mass a specific gravity equal to that of water, is a strong assumption. Near its surface the heated gases can scarcely be supposed to have so high a specific gravity, and, if not, the interior must be supposed to have a much higher specific gravity. Again, M. Faye's hypothesis appears to be espoused by him, partly because it affords an explanation of the spots, which are considered as openings in the photosphere, exposing the comparatively non-luminous gases

filling the interior. But if these interior gases are non-luminous from the absence of precipitated matter, must they not for the same reason be transparent? And if transparent, will not the light from the remote side of the photosphere seen through them be nearly as bright as that of the side next to us? By as much as the intensely-heated gases of the interior are disabled by the dissociation of their molecules from giving off luminiferous undulations, by so much must they be disabled from absorbing the light transmitted through them. And if their great light-transmitting power is exactly complementary to their small light-emitting power, there seems no reason why the interior of the sun, disclosed to us by openings in the photosphere, should not appear as bright as its exterior.

Take now the supposition that a more advanced state of concentration has been reached. A shell of molten metallic matter enclosing a gaseous nucleus still higher in temperature than itself, and ever giving off, in the shape of heat, that motion which the molecules of the whole mass lose as they approach the common centre of gravity, will be continually raised to the highest temperature consistent with its state of liquid aggregation. Unless we assume that simple radiation suffices to give off all the heat generated by progressive integration, we must conclude that the mass will be raised to that temperature at which part of its heat is absorbed in vaporizing its superficial parts. The atmosphere of metallic gases hence resulting cannot continue to accumulate without eventually reaching a height above the sun's surface, at which the cooling caused by radiation and rarefaction will cause condensation into a cloud—cannot, indeed, cease accumulating until the precipitation from the upper limit of the atmosphere balances the evaporation from its lower limit. This upper limit of the atmosphere of metallic gases, whence precipitation is

perpetually taking place, will form the visible photosphere —partly giving off light of its own, partly letting through the more brilliant light of the incandescent mass below. This conclusion harmonizes with the appearances. Sir John Herschel, advocating though he does an antagonist hypothesis, gives a description of the sun's surface which agrees very completely with the processes here supposed. He says:

"There is nothing which represents so faithfully this appearance as the slow subsidence of some flocculent chemical precipitation into a transparent fluid, when viewed perpendicularly from above; so faithfully indeed, that it is hardly possible not to be impressed with the idea of a luminous medium intermixed, but not confounded, with a transparent and non-luminous atmosphere, either floating as clouds in our air, or pervading it in vast sheets and columns like flame or the streamers of our northern lights, directed in lines perpendicular to the surface."

If the constitution of the sun be that which is above inferred, it does not seem difficult to conceive still more specifically the production of these appearances. Everywhere, throughout the atmosphere of metallic vapors which clothes the solar surface, there must be ascending and descending currents. The magnitude of these currents will obviously depend on the depth of this atmosphere; if it is shallow, the currents will be small; but if many thousands of miles deep, the currents may be wide enough to render visible to us the place at which they impinge on the limit of the atmosphere, and the places whence the descending currents commence. The top of an ascending current will be a space over which the thickness of condensed cloud is the least, and through which the greatest amount of light from beneath penetrates. The clouds perpetually formed at the top of such a current will be perpetually thrust

aside by the uncondensed gases from below them; and, growing while they are thrust aside, will collect in the spaces between the ascending currents, where there will result the greatest degree of opacity. Hence the mottled appearance—hence the "pores" or dark interspaces separating the light-giving spots.

Of the more special appearances which the photosphere presents, let us take first the faculæ. These are ascribed to waves in the photosphere; and the way in which such waves might produce an excess of light has been variously explained in conformity with various hypotheses. What would result from them in a photosphere constituted and conditioned as above supposed? Traversing a canopy of cloud, here thicker and there thinner, a wave would cause a disturbance very unlikely to leave the thin and thick parts without any change in their average permeability to light. There would probably be, at some parts of the wave, extensions in the areas of the light-transmitting clouds resulting in the passage of more rays from below. Another phenomenon, less common but more striking, appears also to be in harmony with the hypothesis. I refer to those spots, of a brilliancy much greater than that of the photosphere, which are sometimes observed. In the course of a physical process so vast and so active as that here supposed to be going on in the sun, we may expect that concurrent causes will occasionally produce ascending currents much hotter than usual, or more voluminous, or both. One of these, on reaching the stratum of luminous and illuminated cloud forming the photosphere, will burst through it, dispersing and dissolving it, and ascending to a greater height before it begins itself to condense; meanwhile allowing to be seen, through its transparent mass, the incandescent molten shell of the sun's body.

But what of the spots commonly so called? it will be

asked. In the essay from which the above passages are quoted, it was suggested that refraction of the light, passing through the depressed centres of cyclones in this atmosphere of metallic gases, might possibly be the cause; but this, though defensible as a "true cause," appeared on further consideration to be an inadequate cause. Keeping the question in mind, however, and still taking as a postulate the conclusion of Sir John Herschel, that the spots are in some way produced by cyclones, I was led, in the course of the year following the publication of the essay, to an hypothesis which seemed more satisfactory. This, which I named at the time to Prof. Tyndall, had a point in common with the one afterward published by Prof. Kirchhoff, in so far as it supposed cloud to be the cause of darkness; but differed in so far as the cause of the cloud was assigned. More pressing matters prevented me from developing the idea for some time; and, afterward, I was deterred from including it in the revised edition of the essay, by its inconsistency with the "willow-leaf" doctrine, at that time dominant. The reasoning was as follows: The central region of a cyclone must be a region of rarefaction, and consequently a region of refrigeration. In an atmosphere of metallic gases rising from a molten surface, and presently reaching a limit at which condensation takes place, the molecular state, especially toward its upper part, must be such that a moderate diminution of density, and fall of temperature, will cause precipitation. That is to say, the rarefied interior of a solar cyclone will be filled with cloud; condensation, instead of taking place only at the level of the photosphere, will here extend to a great depth below it, and over a wide area. What will be the characters of a cloud, thus occupying the interior of a cyclone? It will have a rotatory motion; and this it has been seen to have. Being funnel-shaped, as analogy war-

rants us in assuming its central parts will be much deeper than its peripheral parts, and therefore more opaque. This, too, corresponds with observation. Mr. Dawes has discovered that in the middle of the spot there is a blacker spot; just where there would exist a funnel-shaped prolongation of the cyclonic cloud down toward the sun's body, the darkness is greater than elsewhere. Moreover, there is furnished no adequate reason for the depression which one of these dark spaces exhibits. In a whirlwind, as in a whirlpool, the vortex will be below the general level, and all around the surface of the medium will descend toward it. Hence, a spot seen obliquely, as when carried toward the sun's limb, will have its umbra more and more hidden, while its penumbra still remains visible. Nor are we without some interpretation of the penumbra. If, as is implied by what has been said, the so-called "willow-leaves," or "rice-grains," are the tops of the currents ascending from the sun's body, what changes of appearance are they likely to undergo in the neighborhood of a cyclone? For some distance round a cyclone there will be a drawing in of the superficial gases toward the vortex. All the luminous spaces of more transparent cloud forming the adjacent photosphere will be changed in shape by these centripetal currents; they will be greatly elongated; and there will so be produced that "thatch"-like aspect which the penumbra presents.

Of course these views are to be regarded simply as speculative, in common with all others at present current respecting the sun's structure. But, in the absence of any hypothesis supported by something like scientific proof, it has seemed to me well to suggest this one as being warranted by established physical principles, and having a general congruity with the appearances.

X.

MR. MARTINEAU ON EVOLUTION.

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The article by Mr. Martineau, in the April number of the *Contemporary Review*, on "The Place of Mind in Nature, and Intuition of Man," recalled to me a partially-formed intention to deal with the chief criticisms that have from time to time been made on the general doctrine set forth in "First Principles;" since, though not avowedly directed against propositions asserted or implied in that work, Mr. Martineau's reasoning tells against them by implication. The fulfilment of this intention I should, however, have continued to postpone, had I not learned that the arguments of Mr. Martineau are supposed by many to be conclusive, and that, in the absence of replies, it will be assumed that no replies can be made. It seems desirable, therefore, to notice these arguments at once—especially as the essential ones may, I think, be effectually dealt with in a comparatively small space.

The first definite objection which Mr. Martineau raises is, that the hypothesis of General Evolution is powerless to account even for the simpler orders of facts in the absence of numerous different substances. He argues that, were matter all of one kind, no such phenomena as chemical changes would be possible; and that, "in order to start the world on its chemical career, you must enlarge its

capital, and present it with an outfit of *heterogeneous* constituents. Try, therefore, the effect of such a gift; fling into the preëxisting caldron the whole list of recognized elementary substances, and give leave to their affinities to work." The intended implication obviously is, that there must exist the separately-created elements before evolution can begin.

Here, however, Mr. Martineau makes an assumption which few, if any, chemists will commit themselves to, and which many will distinctly deny. There are no "recognized elementary substances," if the expression means substances known to be elementary. What chemists, for convenience, call elementary substances, are merely substances which they have thus far failed to decompose; but, bearing in mind past experiences, they do not dare to say that they are absolutely undecomposable. Water was taken to be an element for more than two thousand years, and then was proved to be a compound; and, until Davy brought a galvanic current to bear upon them, the alkalies and the earths were supposed to be elements. So little true is it that "recognized elementary substances" are supposed to be absolutely elementary, that there has been much speculation among chemists respecting the process of compounding and recompounding by which they have been formed out of some ultimate substance—some chemists having supposed the atom hydrogen to be the unit of composition, but others having contended that the atomic weights of the so-called elements are not thus interpretable. If I remember rightly, Sir John Herschel was one, among others, who, some five-and-twenty years ago, threw out suggestions respecting a system of compounding that might explain these relations of the atomic weights.

What was at that time a suspicion has now become practically a certainty. Spectrum analysis yields results

wholly irreconcilable with the assumption that the conventionally-named simple substances are really simple. Each yields a spectrum having lines varying in number from two to eighty or more, every one of which implies the intercepting of ethereal undulations of a certain order by something oscillating in unison or in harmony with them. Were iron absolutely elementary, it is not conceivable that its atom could intercept ethereal undulations of eighty different orders: though it does not follow that its molecule contains as many separate atoms as there are lines in its spectrum, it must clearly be a complex molecule. Still more clearly is this general implication confirmed by facts furnished by nitrogen; the spectrum of which has two quite different sets of lines, and changes from one set to the other as the temperature is varied. The evidence thus gained points to the conclusion that, out of some primordial units, the so-called elements arise by compounding and recompounding; just as by the compounding and recompounding of so-called elements there arise oxides, and acids, and salts.

And this hypothesis is entirely in harmony with the phenomena of allotropy. Various substances, conventionally distinguished as simple, have several forms under which they present quite different properties. The semi-transparent, colorless, extremely active substance commonly called phosphorus may be so changed as to become opaque, dark red, and inert. Like changes are known to occur in some gaseous, non-metallic elements, as oxygen; and also in metallic elements, as antimony. These total changes of properties, brought about without any changes to be called chemical, are interpretable only as due to molecular rearrangements; and, by showing that difference of property is producible by difference of arrangement, they support the inference otherwise to be drawn,

that the properties of different elements result from differences of arrangement arising by the compounding and re-compounding of ultimate homogeneous units.

Thus Mr. Martineau's objection, which at best would imply a turning of our ignorance of the nature of elements into positive knowledge that they are simple, is, in fact, to be met by two sets of evidences, which distinctly imply that they are compound.

Mr. Martineau next alleges that a fatal difficulty is put in the way of the General Doctrine of Evolution by the existence of a chasm between the living and the not-living. He says: "But with all your enlargement of data, turn them as you will, at the end of every passage which they explore, the *door of life* is closed against them still." Here again our ignorance is employed to play the part of knowledge: the fact that we do not know distinctly how an alleged transition has taken place is transformed into the fact that no transition has taken place. We have, in a more general shape, the argument which until lately was thought conclusive—the argument that because the genesis of each species of creature had not been explained, therefore each species must have been separately created.

Merely noting this, however, I go on to remark that scientific discovery is day by day narrowing the chasm, or, to vary Mr. Martineau's metaphor, "opening the door." Not many years since, it was held as certain that the chemical compounds distinguished as organic could not be formed artificially. Now, more than a thousand organic compounds have been formed artificially. Chemists have discovered the art of building them up, from the simpler to the more complex, and do not doubt that they will eventually produce the most complex. Moreover, the phenomena attending isomeric change give a clew to those

movements which are the only indications we have of life in its lowest forms. In various colloidal substances, including the albuminoid, isomeric change is accompanied by contraction or expansion, and consequent motion; and, in such primordial types as the *Protogenes* of Haeckel, which do not differ in appearance from minute portions of albumen, the observed motions are comprehensible as accompanying isomeric changes caused by variations in surrounding physical actions. The probability of this interpretation will be seen on remembering the evidence we have that, in the higher organisms, many functions are essentially effected by isomeric changes from one to another of the multitudinous forms which protein assumes.

Thus the reply to this objection is, first, that there is going on from both sides a rapid narrowing of the chasm supposed to be impassable; and, second, that, even were the chasm not in course of being filled up, we should no more be justified in therefore assuming a supernatural commencement of life than Kepler was justified in assuming that there were guiding-spirits to keep the planets in their orbits, because he could not see how else they were to be kept in their orbits.

The third definite objection made by Mr. Martineau is of kindred nature. The Hypothesis of Evolution is, he thinks, met by the insurmountable difficulty that plant-life and animal life are absolutely distinct. "You cannot," he says, "take a single step toward the deduction of sensation and thought: neither at the upper limit do the highest plants (the exogens) transcend themselves and overbalance into animal existence; nor at the lower, grope as you may among the sea-weeds and sponges, can you persuade the sporules of the one to develop into the other."

This is an extremely unfortunate objection to raise.

For, though there are no transitions from vegetal to animal life at the places Mr. Martineau names, where, indeed, no biologist would look for them, yet the connection between the two great kingdoms of living things is so complete that separation is now regarded as impossible. For a long time naturalists endeavored to frame definitions such as would, the one include all plants and exclude all animals, and the other include all animals and exclude all plants. But they have been so repeatedly foiled in the attempt that they have given it up. There is no chemical distinction that holds; there is no structural distinction that holds; there is no functional distinction that holds; there is no distinction as to mode of existence that holds. Large groups of the simpler animals contain chlorophyll, and decompose carbonic acid under the influence of light as plants do. Large groups of the simpler plants, as you may observe in the diatoms from any stagnant pool, are no less actively locomotive than the minute creatures classed as animals seen along with them; nay, among these lowest types of living things it is common for the life to be now predominantly animal and presently to become predominantly vegetal. The very name *zoospores*, given to germs of *algæ*, which for a while swim about actively by means of cilia, and presently settling down grow into plant-forms, is given because of this conspicuous community of nature. So complete is this community of nature that for some time past many naturalists have wished to establish for these lowest types a sub-kingdom intermediate between the animal and the vegetal: the reason against this course being, however, that the difficulty crops up afresh at any assumed places where this intermediate sub-kingdom may be supposed to join the other two.

Thus the assumption on which Mr. Martineau proceeds

is diametrically opposed to the conviction of naturalists in general.

Though I do not perceive that it is specifically stated, there appears to be tacitly implied a fourth difficulty of allied kind—the difficulty that there is no possibility of transition from life of the simplest kind to mind. Mr. Martineau says, indeed, that there can be "with only vital resources, as in the vegetable world, no beginning of mind:" apparently leaving it to be inferred that in the animal world the resources are such as to make the "beginning of mind" comprehensible. If, however, instead of leaving it a latent inference, he had distinctly asserted a chasm between mind and bodily life, for which there is certainly quite as much reason as for asserting a chasm between animal life and vegetal life, the difficulties in his way would have been no less insuperable.

For those lowest forms of irritability in the animal kingdom, which, I suppose, Mr. Martineau refers to as the "beginning of mind," are not distinguishable from the irritability which plants display: they in no greater degree imply consciousness. If the sudden folding of a sensitive-plant's leaf when touched, or the spreading out of the stamens in a wild-cistus when gently brushed, is to be considered a vital action of a purely physical kind, then so too must be considered the equally slow contraction of a polype's tentacles. And yet, from this simple motion of an animal having no nervous system, we may pass by insensible stages through ever-complicating forms of actions, with their accompanying signs of feeling and intelligence, until we reach the highest.

Even apart from the evidence derived from the ascending grades of animals up from *zoophytes*, as they are significantly named, it needs only to observe the evolution

of a single animal to see that there does not exist any break or chasm between the life which shows no mind and the life which shows mind. The yolk of an egg which the cook has just broken not only yields no sign of mind, but yields no sign of life. It does not respond to a stimulus as much even as many plants do. Had the egg, instead of being broken by the cook, been left under the hen for a certain time, the yolk would have passed by infinitesimal gradations through a series of forms ending in the chick, and by similarly infinitesimal gradations would have arisen those functions which end in the chick breaking its shell; and which, when it gets out, show themselves in running about, distinguishing and picking up food, and squeaking if hurt. When did the feeling begin, and how did there come into existence that power of perception which the chick's actions show? Should it be objected that the chick's actions are mainly automatic, I will not dwell on the fact that, though they are largely so, the chick manifestly has feeling and therefore consciousness, but I will accept the objection, and propose that instead we take the human being. The course of development before birth is just of the same general kind; and similarly, at a certain stage, begins to be accompanied by reflex movements. At birth there is displayed an amount of mind certainly not greater than that of the chick—there is no power of running from danger, no power of distinguishing and picking up food. If we say the chick is unintelligent, we must certainly say the infant is unintelligent. And yet from the unintelligence of the infant to the intelligence of the adult, there is an advance by steps so small that on no day is the amount of mind shown appreciably different from that shown on preceding and succeeding days.

Thus the tacit assumption, that there exists a break, is

not simply gratuitous, but is negatived by the most obvious facts.

Certain of the words and phrases, used in explaining that particular part of the Doctrine of Evolution which deals with the origin of species, are commented upon by Mr. Martineau as having implications justifying his view. Let us consider his comments.

He says that *competition* is not an "original power, which can of itself do any thing;" further, that "it cannot act except in the presence of some *possibility of a better or worse;*" and that this "possibility of a better or worse" implies a "world prearranged for progress," "a directing Will intent upon the good." Had Mr. Martineau looked more closely into the matter, he would have found that, though the words and phrases he quotes are used for convenience, the conceptions they imply are not at all essential to the doctrine. Under its rigorously-scientific form, the doctrine is expressible in purely-physical terms, which neither imply competition nor imply better and worse.*

Beyond this indirect mistake there is a direct mistake. Mr. Martineau speaks of the "survivorship of the better," as though that were the statement of the law, and then adds that the alleged result cannot be inferred "except on the assumption that whatever is *better* is *stronger* too." But the words he here uses are his own words, not the words of those he opposes. The law is the survival of the *fittest.* Probably, in substituting "better" for "fittest," Mr. Martineau did not suppose that he was changing the meaning; though I dare say he perceived that the meaning of the word "fittest" did not suit his argument so well. Had he examined the facts, he would have found

* "Principles of Biology," §§ 159–168.

that the law is not the survival of the "better" or the "stronger," if we give to those words any thing like their ordinary meanings. It is the survival of those which are constitutionally fittest to thrive under the conditions in which they are placed; and very often that which, humanly speaking, is inferiority, causes the survival. Superiority, whether in size, strength, activity, or sagacity, is, other things equal, at the cost of diminished fertility; and where the life led by a species does not demand these higher attributes, the species profits by decrease of them, and accompanying increase of fertility. This is the reason why there occur so many cases of retrograde metamorphosis—this is the reason why parasites, internal and external, are so commonly degraded forms of higher types. Survival of the "better" does not cover these cases, though survival of the "fittest" does. When it is remembered that these cases outnumber all others—that there are more species of parasites than there are species of all other animals put together—it will be seen that the expression "survivorship of the better" is wholly inappropriate, and the argument Mr. Martineau bases upon it quite untenable. Indeed, if, in place of those adjustments of the human sense-organs, which he so eloquently describes as implying prearrangement, Mr. Martineau had described the countless elaborate appliances which enable parasites to torture animals immeasurably superior to them, and which, from his point of view, no less imply prearrangement, I think the notes of admiration which end his descriptions would not have seemed to him or his readers so appropriate.

One more word there is from the intrinsic meaning of which Mr. Martineau deduces what appears a powerful argument — the word *Evolution* itself. He says:

"It means, to unfold from within; and it is taken from the history of the seed or embryo of living natures. And what is the seed but a casket of prearranged futurities, with its whole contents *prospective*, settled to be what they are by reference to ends still in the distance?"

Now, this criticism would have been very much to the point did the word Evolution truly express the process it names. If this process, as scientifically defined, really involved that conception which the word evolution was originally designed to convey, the implications would be those Mr. Martineau alleges. But, unfortunately for him, the word, having been in possession of the field before the process was understood, has been adopted merely because displacing it by another word seemed impracticable. And this adoption of it has been joined with a caution against misunderstandings arising from its unfitness. Here is a part of the caution: "Evolution has other meanings, some of which are incongruous with, and some even directly opposed to, the meaning here given to it. . . . The antithetical word, Involution, would much more truly express the nature of the process; and would, indeed, describe better the secondary characters of the process which we shall have to deal with presently."* So that the meanings which the word involves, and which Mr. Martineau regards as fatal to the hypothesis, are already repudiated as not belonging to the hypothesis.

And now, having dealt with the essential objections raised by Mr. Martineau to the Hypothesis of Evolution as it is presented under that purely scientific form which generalizes the process of things, firstly as observed, and secondly as inferred from certain ultimate principles, let me go on to examine that form of the Hypothesis which

* "First Principles," second edition, § 97.

he propounds—Evolution as determined by Mind and Will—Evolution as prearranged by a Divine Actor. For Mr. Martineau apparently abandons the primitive theory of creation by "fiat of Almighty Will" and also the theory of creation by manufacture—by "a contriving and adapting power," and seems to believe in Evolution; requiring only that "an originating Mind" shall be taken as its antecedent. Let us ask, first, in what relation Mr. Martineau conceives the "originating Mind" to stand to the evolving universe. From some passages it is inferable that he considers the "presence of mind" to be everywhere needful. He says:

"It is impossible to work the theory of Evolution upward from the bottom. If all force is to be conceived as one, its type must be looked for in the highest and all-comprehending term; and Mind must be conceived as there, and as divesting itself of some specialty at each step of its descent to a lower stratum of law, till represented at the base under the guise of simple Dynamics."

This seems to be an unmistakable assertion that, wherever Evolution is going on, Mind is then and there behind it. At the close of the argument, however, a quite different conception is implied. Mr. Martineau says:

"If the Divine Idea will not retire at the bidding of our speculative science, but retains its place, it is natural to ask, What is its relation to the series of so-called Forces in the world? But the question is too large and deep to be answered here. Let it suffice to say, that there need not be any *overruling* of these forces by the Will of God, so that the supernatural should disturb the natural; or any *supplementing* of them, so that He should fill up their deficiencies. Rather is His thought related to them as, in man, the mental force is related to all below it."

It would take too much space to deal fully with the various questions which this last passage raises. There is the question, Whence come these "Forces," spoken of as

separate from the "Will of God"—did they preëxist? Then what becomes of the divine power? Do they exist by the divine Will? Then what kind of nature is that by which they act apart from the divine Will? Again, there is the question, How do these deputy-forces coöperate in each particular phenomenon, if the presiding Will is not there present to control them? Either an organ, which develops into fitness for its function, develops by the coöperation of these forces under the direction of Mind then present, or it so develops in the absence of Mind. If it develops in the absence of Mind, the hypothesis is given up; and if the "originating Mind" is required to be then and there present, we must suppose a particular providence to be present in each particular organ of each particular creature throughout the universe. Once more there is the question, If "His thought is related to them [these Forces] as, in Man, the mental force is related to all below it," how can "His thought" be regarded as the cause of Evolution? In man the mental force is related to the forces below it neither as a creator of them, nor as a regulator of them, save in a very limited way: the greater part of the forces present in man, both structural and functional, defy the mental force absolutely. Nay, more, it needs but to injure a nerve to see that the power of the mental force over the physical forces is dependent on conditions that are themselves physical, and one who takes morphia, in mistake for magnesia, discovers that the power of the physical forces over the mental is *un*conditioned by any thing mental.

Not dwelling on these questions, however, I will merely draw attention to the entire incongruity of this conception with the previous conception which I have quoted. Assuming that, when the choice is pressed on him, Mr. Martineau will choose the first, which alone has any thing

like defensibility, let us go on to ask how far Evolution is made more comprehensible by postulating Mind, universally immanent, as its cause.

In metaphysical controversy, many of the propositions propounded and accepted as quite believable are absolutely inconceivable. There is a perpetual confusing of actual ideas with what are nothing but pseud-ideas. No distinction is made between propositions that contain real thoughts, and propositions that are only the forms of thoughts. A thinkable proposition is one of which *the two terms can be brought together in consciousness under the relation said to exist between them.* But very often, when the subject of a proposition has been thought of as something known, and when the predicate has been thought of as something known, and when the relation alleged between them has been thought of as a known relation, it is supposed that the proposition itself has been thought. The thinking separately of the elements of a proposition is mistaken for the thinking of them in the combination which the proposition affirms. And hence it continually happens that propositions which cannot be rendered into thought at all are supposed to be not only thought but believed. The proposition that Evolution is caused by Mind is one of this nature. The two terms are separately intelligible; but they can be regarded in the relation of effect and cause only so long as no attempt is made to put them together in this relation.

The only thing which any one knows as Mind is the series of his own states of consciousness; and if he thinks of any mind other than his own, he can think of it only in terms derived from his own. If I am asked to frame a notion of Mind, divested of all those structural traits under which alone I am conscious of mind in myself, I cannot do it. I know nothing of thought save as carried on in

ideas originally traceable to the effects wrought by objects on me. A mental act is an unintelligible phrase if I am not to regard it as an act in which states of consciousness are severally known as like other states in the series that has gone by, and in which the relations between them are severally known as like past relations in the series. If, then, I have to conceive Evolution as caused by an "originating Mind," I must conceive this Mind as having attributes akin to those of the only mind I know, and without which I cannot conceive mind at all.

I will not dwell on the many incongruities hence resulting, by asking how the "originating Mind" is to be thought of as having states produced by things objective to it; as discriminating among these states, and classing them as like and unlike; and as preferring one objective result to another. I will simply ask, What happens if we ascribe to the "originating Mind" the character absolutely essential to the conception of mind, that it consists of a series of states of consciousness? Put a series of states of consciousness as cause, and the evolving universe as effect, and then endeavor to see the last as flowing from the first. I find it possible to imagine in some dim way a series of states of consciousness serving as antecedent to any one of the movements I see going on; for my own states of consciousness are often indirectly the antecedents to such movements. But how if I attempt to think of such a series as antecedent to *all* actions throughout the universe—to the motions of the multitudinous stars through space, to the revolutions of all their planets round them, to the gyrations of all these planets on their axes, to the infinitely-multiplied physical processes going on in each of these suns and planets? I cannot think of a single series of states of consciousness as causing even the relatively small group of actions going on over the earth's surface.

I cannot think of it even as antecedent to all the various winds and the dissolving clouds they bear, to the currents of all the rivers, and the grinding actions of all the glaciers; still less can I think of it as antecedent to the infinity of processes simultaneously going on in all the plants that cover the globe, from scattered polar lichens to crowded tropical palms, and in all the millions of quadrupeds that roam among them, and the millions of millions of insects that buzz about them. Even to a single small set of these multitudinous terrestrial changes, I cannot conceive as antecedent a single series of states of consciousness—cannot, for instance, think of it as causing the hundred thousand breakers that are at this instant curling over on the shores of England. How, then, is it possible for me to conceive an "originating Mind," which I must represent to myself as a *single* series of states of consciousness, working the infinitely-multiplied sets of changes *simultaneously* going on in worlds too numerous to count, dispersed throughout a space that baffles imagination?

If, to account for this infinitude of physical changes everywhere going on, "Mind must be conceived as there" "under the guise of simple Dynamics," then the reply is that, to be so conceived, Mind must be divested of all attributes by which it is distinguished; and that, when thus divested of its distinguishing attributes, the conception disappears—the word Mind stands for a blank. If Mr. Martineau takes refuge in the entirely different and, as it seems to me, incongruous hypothesis of something like a plurality of minds—if he accepts, as he seems to do, the doctrine that you cannot explain Evolution "unless among your primordial elements you scatter already the *germs* of Mind as well as the inferior elements"—if the insuperable difficulties I have just pointed out are to be met by assuming a local series of states of consciousness for each phenom-

enon, then we are obviously carried back to something like the old fetichistic notion, with the difference only, that the assumed spiritual agencies are indefinitely multiplied.

Clearly, therefore, the proposition that an "originating Mind" is the cause of Evolution is a proposition that can be entertained so long only as no attempt is made to unite in thought its two terms in the alleged relation. That it should be accepted as a matter of *faith*, may be a defensible position, provided good cause is shown why it should be so accepted; but that it should be accepted as a matter of *understanding*—as a statement making the order of the universe comprehensible—is a quite indefensible position.

Here let me guard myself against a misinterpretation very likely to be put upon the foregoing arguments—especially by those who have read the Essay to which they reply. The statements of that Essay carry the implication that all who adhere to the hypothesis it combats imagine they have solved the mystery of things when they have shown the processes of Evolution to be naturally caused. Mr. Martineau tacitly represents them as believing that, when every thing has been interpreted in terms of Matter and Motion, nothing remains to be explained. This, however, is by no means the fact. The Doctrine of Evolution, under its purely scientific form, does not involve Materialism, though its opponents persistently represent it as doing so. Indeed, among adherents of it who are friends of mine, there are those who speak of the Materialism of Buchner and his school, with a contempt certainly not less than that felt by Mr. Martineau. To show how anti-materialistic my own view is, I may, perhaps, without impropriety, quote some out of many passages which I have written on the question elsewhere:

"Hence, though of the two it seems easier to translate so-called Matter into so-called Spirit, than to translate so-called Spirit into so-called Matter (which latter is, indeed, wholly impossible), yet no translation can carry us beyond our symbols." *

And again:

"See, then, our predicament. We can think of Matter only in terms of Mind. We can think of Mind only in terms of Matter. When we have pushed our explorations of the first to the uttermost limit, we are referred to the second for a final answer; and, when we have got the final answer of the second, we are referred back to the first for an interpretation of it. We find the value of *x* in terms of *y*; then we find the value of *y* in terms of *x*; and so on we may continue forever, without coming nearer to a solution. The antithesis of subject and object, never to be transcended while consciousness lasts, renders impossible all knowledge of that Ultimate Reality in which subject and object are united." †

It is thus, I think, manifest that the difference between Mr. Martineau's view and the view he opposes is by no means so wide as he makes it appear; and further, it seems to me that such difference as exists is rather the reverse of that indicated by his exposition. Briefly expressed, the difference is that, where he thinks there is no mystery, the doctrine he combats recognizes a mystery. Speaking for myself only, I may say that, agreeing entirely with Mr. Martineau in repudiating the materialistic interpretation as utterly futile, I differ from him simply in this, that while he says he has found another interpretation, I confess that I cannot find any interpretation; while he holds that he can understand the Power which is manifested in things, I feel obliged to admit, after many failures, that I cannot understand it. So that, in presence of the transcendent problem which the universe presents, Mr. Martineau regards the human intellect as capable, and

* "Principles of Psychology," second edition, vol. i., § 63.

† Ibid, § 272.

I as incapable. This contrast does not appear to me of the kind which his Essay tacitly asserts. If there is such a thing as the "pride of Science," it is obviously exceeded by the pride of Theology. I fail to perceive humility in the belief that the human mind is able to comprehend that which is behind appearances; and I do not see how piety is especially exemplified in the assertion that the Universe contains no mode of existence higher in Nature than that which is present to us in consciousness. On the contrary, I think it quite a defensible proposition that humility is better shown by a confession of incompetence to grasp in thought the Cause of all things; and that the religious sentiment may find its highest sphere in the belief that the Ultimate Power is no more representable in terms of human consciousness than human consciousness is representable in terms of a plant's functions.

Other parts of Mr. Martineau's argument I pass over as being met by implication in the above replies. I will now add only that, should any further explanation be required, I must postpone it until I am free from present special engagements.

XI.

REPLIES TO CRITICISMS.

[FROM THE FORTNIGHTLY REVIEW FOR NOVEMBER AND DECEMBER, 1873.]

REPLIES TO CRITICISMS.

When made by a competent reader, an objection usually implies one of two things. Either the statement to which he demurs is wholly or partially untrue; or, if true, it is presented in such a way as to permit misapprehension. A need for some change or addition is in any case shown.

Not recognizing the errors alleged, but thinking rather that misapprehensions cause the dissent of those who have attacked the metaphysico-theological doctrines held by me, I propose here to meet, by explanations and arguments, the chief objections urged: partly with the view of justifying these doctrines, and partly with the view of guarding against the wrong interpretations which it appears are apt to be made.

The pages of a periodical intended for general reading may be thought scarcely fitted for the treatment of these highly abstract questions. There is now, however, so considerable a class interested in them, and they are so deeply involved with the great changes of opinion in progress, that I have ventured to hope for readers outside the circle of those who occupy themselves with philosophy.

Of course the criticisms to be noticed I have selected, either because of their intrinsic force, or because they come from men whose positions or reputations give them

weight. To meet more than a few of my opponents is out of the question.

Let me begin with a criticism contained in the sermon preached by the Rev. Principal Caird before the British Association, on the occasion of its meeting in Edinburgh, in August, 1871. Expressed with a courtesy which, happily, is now less rare than of yore in theological controversy, Dr. Caird's objection might, I think, be admitted without involving essential change in the conclusion demurred to; while it might be shown to tell with greater force against the conclusions of thinkers classed as orthodox, Sir W. Hamilton and Dean Mansel, than against my own. Describing this as set forth by me, Dr. Caird says:—

"His thesis is that the provinces of science and religion are distinguished from each other as the known from the unknown and unknowable. This thesis is maintained mainly on a critical examination of the nature of human intelligence, in which the writer adopts and carries to its extreme logical results the doctrine of the relativity of human knowledge which, propounded by Kant, has been reproduced with special application to theology by a famous school of philosophers in this country. From the very nature of human intelligence, it is attempted to be shown that it can only know what is finite and relative, and that therefore the absolute and infinite the human mind is, by an inherent and insuperable disability, debarred from knowing. . . . . May it not be asked, for one thing, whether in the assertion, as the result of an examination of the human intellect, that it is incapable of knowing what lies beyond the finite, there is not involved an obvious self-contradiction? The examination of the mind can be conducted only by the mind, and if the instrument be, as is alleged, limited and defective, the result of the inquiry must partake of that defectiveness. Again, does not the knowledge of a limit imply already the power to transcend it? In affirming that human science is incapable of crossing the bounds of the finite world, is it not a necessary presupposition that you who so affirm have crossed these bounds?"

That this objection is one I am not disinclined to recog-

nize, will be inferred when I state that it is one I have myself raised. While preparing the second edition of the *Principles of Psychology*, I found, among my memoranda, a note which still bore the wafers by which it had been attached to the original manuscript (unless, indeed, it had been transferred from the MS. of *First Principles*, which its allusion seems to imply). It was this:—

"I may here remark in passing that the several reasonings, including the one above quoted, by which Sir William Hamilton would demonstrate the pure relativity of our knowledge—reasonings which clearly establish many important truths, and with which in the main I agree—are yet capable of being turned against himself, when he definitely concludes that it is impossible for us to know the absolute. For to positively assert that the absolute cannot be known, is in a certain sense to assert a *knowledge* of it—is to *know* it as *unknowable*. To affirm that human intelligence is confined to the conditioned, is to put an *absolute limit* to human intelligence, and implies *absolute knowledge*. It seems to me that the 'learned ignorance' with which philosophy ends, must be carried a step further; and instead of positively saying that the absolute is unknowable, we must say that we cannot tell whether it is knowable or not."

Why I omitted this note I cannot now remember. Possibly it was because re-consideration disclosed a reply to the contained objection. For while it is true that the intellect cannot prove its own competence, since it must postulate its own competence in the course of the proof, and so beg the question; yet it does not follow that it cannot prove its own incompetence respecting questions of certain kinds. Its inability in respect of such questions has two conceivable causes. It may be that the deliverances of Reason in general are invalid, in which case the incompetence of Reason to solve questions of a certain class is implied by its general incompetence; or it may be that the deliverances of Reason, valid within a certain range, themselves end in the conclusion that

Reason is incapable beyond that range. So that while there can be no proof of competence, because competence is postulated in each step of the demonstration, there may be proof of incompetence either (1) if the successive deliverances forming the steps of the demonstration, by severally evolving contradictions, show their untrustworthiness, or (2) if, being trustworthy, they lead to the result that on certain questions Reason cannot give any deliverance.

Reason leads both inductively and deductively to the conclusion that the sphere of Reason is limited. Inductively, this conclusion expresses the result of countless futile attempts to transcend this sphere—attempts to understand Matter, Motion, Space, Time, Force, in their ultimate natures—attempts which, bringing us always to alternative impossibilities of thought, warrant the inference that such attempts will continue to fail, as they have hitherto failed. Deductively, this conclusion expresses the result of mental analysis, which shows us that the product of thought is in all cases a relation, identified as such or such; that the process of thought is the identification and classing of relations; that therefore Being in itself, out of relation, is unthinkable, as not admitting of being brought within the form of thought. That is to say, deduction explains that failure of Reason established as an induction from many experiments. And to call in question the ability of Reason to give this verdict against itself, in respect of these transcendent problems, is to call in question its ability to draw valid conclusions from premises; which is to assert a general incompetence necessarily inclusive of the special incompetence.

Closely connected with the foregoing, is a criticism from

Dr. Mansel, on which I may here make some comments. In a note to his *Philosophy of the Conditioned* (p. 39), he says :—

"Mr. Herbert Spencer, in his work on *First Principles*, endeavours to press Sir W. Hamilton into the service of Pantheism and Positivism together" [a somewhat strange assertion, by the way, considering that I reject them both], "by adopting the negative portion only of his philosophy—in which, in common with many other writers, he declares the absolute to be inconceivable by the mere intellect,—and rejecting the positive portions, in which he most emphatically maintains that the belief in a personal God is imperatively demanded by the facts of our moral and emotional consciousness. . . . . Sir W. Hamilton's fundamental principle is, that consciousness must be accepted entire, and that the moral and religious feelings, which are the primary source of our belief in a personal God, are in no way invalidated by the merely negative inferences which have deluded men into the assumption of an impersonal absolute. . . . . Mr. Spencer, on the other hand, takes these negative inferences as the only basis of religion, and abandons Hamilton's great principle of the distinction between knowledge and belief."

Putting these statements in the order most convenient for discussion, I will deal first with the last of them. Instead of saying what he does, Dr. Mansel should have said that I decline to follow Sir W. Hamilton in confounding two distinct, and indeed radically-opposed, meanings of the word *belief*. This word "is habitually applied to dicta of consciousness for which no proof can be assigned: both those which are unprovable because they underlie all proof, and those which are unprovable because of the absence of evidence."* In the pages of this Review for July, 1865, I exhibited this distinction as follows :—

"We commonly say we 'believe' a thing for which we can assign some preponderating evidence, or concerning which we have received

* *Principles of Psychology*, Second Edition, § 425, note.

some indefinable impression. We *believe* that the next House of Commons will not abolish Church-rates; or we *believe* that a person on whose face we look is good-natured. That is, when we can give confessedly-inadequate proofs, or no proofs at all, for the things we think, we call them 'beliefs.' And it is the peculiarity of these beliefs, as contrasted with cognitions, that their connexions with antecedent states of consciousness may be easily severed, instead of being difficult to sever. But unhappily, the word 'belief' is also applied to each of those temporarily or permanently indissoluble connexions in consciousness, for the acceptance of which the only warrant is that it cannot be got rid of. Saying that I feel a pain, or hear a sound, or see one line to be longer than another, is saying that there has occurred in me a certain change of state; and it is impossible for me to give a stronger evidence of this fact than that it is present to my mind. . . . . 'Belief' having, as above pointed out, become the name of an impression for which we can give only a confessedly-inadequate reason, or no reason at all; it happens that when pushed hard respecting the warrant for any ultimate dictum of consciousness, we say, in the absence of all assignable reason, that we *believe* it. Thus the two opposite poles of knowledge go under the same name; and by the reverse connotations of this name, as used for the most coherent and least coherent relations of thought, profound misconceptions have been generated."

Now that the belief which the moral and religious feelings are said to yield of a personal God, is not one of the beliefs which are unprovable because they underlie all proof, is obvious. It needs but to remember that in works on Natural Theology, the existence of a personal God is *inferred* from these moral and religious feelings, to show that it is not contained in these feelings themselves, or joined with them as an inseparable intuition. It is not a belief like the beliefs which I now have that this is daylight, and that there is open space before me—beliefs which cannot be proved because they are of equal simplicity with, and of no less certainty than, each step in a demonstration. Were it a belief of this most certain kind, argument would be superfluous: all races of men

and every individual would have the belief in an inexpugnable form. Hence it is manifest that, confusing the two very different states of consciousness called beliefs, Sir W. Hamilton ascribes to the second a certainty that belongs only to the first.

Again, neither Sir W. Hamilton nor Dr. Mansel has enabled us to distinguish those "facts of our moral and emotional consciousness" which imperatively demand the belief in a personal God, from those facts of our (or of men's) "moral and emotional consciousness" which, in those having them, imperatively demand beliefs that Sir W. Hamilton would regard as untrue. A New Zealand chief, discovering his wife in an infidelity, killed the man; the wife then killed herself that she might join her lover in the other world; and the chief thereupon killed himself that he might go after them to defeat this intention. These two acts of suicide furnish tolerably strong evidence that these New Zealanders believed in another world to which they could go at will, and fulfil their desires as they did here. If they were asked the justification for this belief, and if the arguments by which they sought to establish it were not admitted, they might still fall back on emotional consciousness as yielding them an unshakable foundation for it. I do not see why a Fiji Islander, adopting the Hamiltonian argument, should not justify by it his conviction that after being buried alive, his life in the other world, forthwith commencing at the age he has reached in this, will similarly supply him with the joys of conquest and the gratifications of cannibalism. That he has a conviction to this effect stronger than the religious convictions current among civilized people, is proved by the fact that he goes to be buried alive quite willingly. And as we may presume that his conviction is not the outcome of a

demonstration, it must be the outcome of some state of feeling—some "emotional consciousness." Why, then, should he not assign the "facts" of his "emotional consciousness" as "imperatively demanding" this belief. Manifestly, this principle that "consciousness must be accepted entire," either obliges us to accept as true the superstitions of all mankind, or else obliges us to say that the consciousness of a certain limited class of cultivated people is alone meant. If things are to be believed simply because the facts of emotional consciousness imperatively demand the beliefs, I do not see why the actual existence of a ghost in a house, is not inevitably implied by the intense fear of it that is aroused in the child or the servant.

Lastly, and chiefly, I have to deal with Dr. Mansel's statement that "Mr. Spencer, on the other hand, takes these negative inferences as the only basis of religion." This statement is exactly the reverse of the truth; since I have contended, against Hamilton and against him, that the consciousness of that which is manifested to us through phenomena is *positive*, and not *negative* as they allege, and that this positive consciousness supplies an indestructible basis for the religious sentiment (*First Principles*, § 26). Instead of giving here passages to show this, I may fitly quote the statement and opinion of a foreign theologian. M. le pasteur Grotz, of the Reformed Church at Nismes, writes thus:—

"La science serait-elle donc par nature ennemie de la religion? pour être religieux, faut-il proscrire la science?—C'est la science, la science expérimentale qui va maintenant parler en faveur de la religion; c'est elle qui, par la bouche de l'un des penseurs . . . de notre époque, M. Herbert Spencer, va répondre à la fois à M. Vacherot et à M. Comte."

* * * * * *

"Ici, M. Spencer discute la théorie de l'*inconditionné*; entendez

par ce mot: Dieu. Le philosophe écossais, Hamilton, et son disciple, M. Mansel, disent comme nos positivistes français: 'Nous ne pouvons affirmer l'existence positive de quoi que ce soit au delà des phénomènes.' Seulement, Hamilton et son disciple se séparent de nos compatriotes en faisant intervenir une 'révélation merveilleuse' qui nous fait croire à l'existence de l'inconditionné, et grâce à cette révélation vraiment merveilleuse, toute l'orthodoxie revient. Est-il vrai que nous ne puissions rien affirmer au delà des phénomènes? M. Spencer déclare qu'il y a dans cette assertion une grave erreur. Le côté logique, dit-il fort justement, n'est pas le seul; il y a aussi le côté psychologique, et, selon nous, il prouve que l'existence positive de l'absolu est une donnée nécessaire de la conscience."

"Là est la base de l'accord entre la religion et la science. Dans un chapitre . . . . intitulé *Réconciliation*, M Spencer etablit et développe cet accord sur son véritable terrain."

* * * * * *

"M. Spencer, en restant sur le terrain de la logique et de la psychologie, et sans recourir à une intervention surnaturelle, a établi la legitimité, la nécessité et l'éternelle durée du sentiment religieux et de la religion."*

I turn next to what has been said by Dr. Shadworth H. Hodgson, in his essay on "The Future of Metaphysic," published in the *Contemporary Review* for November, 1872. Remarking only, with respect to the agreements he expresses in certain views of mine, that I value them as coming from a thinker of subtlety and independence, I will confine myself here to his disagreements. Dr. Hodgson, before giving his own view, briefly describes and criticizes the views of Hegel and Comte, with both of whom he partly agrees and partly disagrees, and then proceeds to criticize the view set forth by me. After a preliminary brief statement of my position, to the wording of which I demur, he goes on to say:—

"In his *First Principles*, Part 1, second ed., there is a chapter

* *Le Sentiment Religieux*, par A. Grotz. Paris, J. Cherbuliez, 1870.

headed 'Ultimate Scientific Ideas,' in which he enumerates six such ideas or groups of ideas, and attempts to show that they are entirely incomprehensible. The six are:—1. Space and Time. 2. Matter. 3. Rest and Motion. 4. Force. 5. Consciousness. 6. The Soul, or the Ego. Now to enter at length into all of these would be an undertaking too large for the present occasion; but I will take the first of the six, and endeavour to show in its case the entire untenability of Mr. Spencer's view; and since the same arguments may be employed against the rest, I shall be content that my case against them should be held to fail if my case should fail in respect to Space and Time."

I willingly join issue with Dr. Hodgson on these terms; and proceed to examine, one by one, the several arguments he uses to show the invalidity of my conclusions. Following his criticisms in the order he has chosen, I begin with the sentence following that which I have just quoted. The first part of it runs thus:—"The metaphysical view of Space and Time is, that they are elements in all phenomena, whether the phenomena are presentations or representations."

Whether, by "the metaphysical view," is here meant the view of Kant, whether it means Dr. Hodgson's own view, or whether the expression has a more general meaning, I have simply to reply that the metaphysical view is incorrect. Dealing with the Kantian version of this doctrine, that Space is a form of intuition, I have pointed out that only with certain classes of phenomena is Space united indissolubly; that Kant habitually considers phenomena belonging to the visual and tactual groups, with which the consciousness of space is inseparably joined, and overlooks groups with which it is not inseparably joined. Though in the adult, perception of sound has certain space-implications, mostly, if not wholly, acquired by individual experience; and though it would seem from the instructive experiments of Mr. Spalding, that in creatures born with nervous systems

much more organized than our own are at birth, there is some innate perception of the side from which a sound comes ; yet it is demonstrable that the space-implications of sound are not originally given with the sensation as its form of intuition. Bearing in mind the Kantian doctrine, that Space is the form of sensuous intuitions not only as *presented* but also as *represented*, let us examine critically our musical ideas. As I have elsewhere suggested to the reader—

"Let him observe what happens when some melody takes possession of his imagination. Its tones and cadences go on repeating themselves apart from any space-consciousness—they are not localized. He may or may not be reminded of the place where he heard them—this association is incidental only. Having observed this, he will see that such space-implications as sounds have, are learnt in the course of individual experience, and are not given with the sounds themselves Indeed, if we refer to the Kantian definition of form, we get a simple and conclusive proof of this. Kant says form is 'that which effects that the content of the phenomenon can be arranged under certain relations.' How then can the content of phenomenon we call sound be arranged? Its parts can be arranged in order of sequence—that is, in Time. But there is no possibility of arranging its parts in order of coexistence—that is, in Space. And it is just the same with odour. Whoever thinks that sound and odour have Space for their form of intuition, may convince himself to the contrary by trying to find the right and left sides of a sound, or to imagine an odour turned the other way upwards."—*Principles of Psychology*, § 399.—Note.

As I thus dissent, not I think without good reason, from "the metaphysical view of Space and Time" as "elements in all phenomena," it will naturally be expected that I dissent from the first criticism which Dr. Hodgson proceeds to deduce from it. Dealing first with the arguments I have used to show the incomprehensibility of Space and Time, if we consider them as objective, and stating in other words the conclusion I draw, that "as Space and Time cannot be either non-

entities nor the attributes of entities, we have no choice but to consider them as entities." Dr. Hodgson continues :—

"So far good. Secondly, he argues that they cannot be represented in thought as such real existences, because 'to be conceived at all, a thing must be conceived as having attributes.' Now here the metaphysical doctrine enables us to conceive them as real existences, and rebuts the argument for their inconceivability; for the other element, the material element, the feeling or quality occupying Space and Time stands in the place and performs the function of the required attributes, composing together with the space and time which is occupied the empirical phenomena of perception. So far as this argument of Mr. Spencer goes, then, we are entitled to say that his case for the inconceivability of Space and Time as real existences is not made out."

Whether the fault is in me or not I cannot say, but I fail to see that my argument is thus rebutted. On the contrary, it appears to me substantially conceded. What kind of entity is that which can exist only when occupied by something else? Dr. Hodgson's own argument is a tacit assertion that Space *by itself* cannot be conceived as an existence; and this is all that I have alleged.

Dr. Hodgson deals next with the further argument, familiar to all readers, which I have added as showing the insurmountable difficulty in the way of conceiving Space and Time as objective entities; namely, that "all entities which we actually know as such are limited. . . . But of Space and Time we cannot assert either limitation, or the absence of limitation." Without quoting at length the reasons Dr. Hodgson gives for distinguishing between Space as *per*ceived and Space as *con*ceived, it will suffice if I quote his own statement of the result to which they bring him: "So that Space and Time as perceived are not finite, but infinite, as conceived, are not infinite, but finite."

Most readers will, I think, be startled by the assertion that conception is less extensive in range than perception; but, without dwelling on this, I will content myself by asking in what case Space is perceived as infinite? Surely Dr. Hodgson does not mean to say that he can perceive the whole surrounding Space at once—that the Space behind is united in perception with the Space in front. Yet this is the necessary implication of his words. Taking his statement less literally, however, and not dwelling on the fact that in perception Space is habitually bounded by objects more or less distant, let us test his assertion under the most favourable conditions. Supposing the eye directed upwards towards a clear sky; is not the space then perceived, laterally limited? The visual area, restricted by the visual apertures, cannot include in perception even 180° from side to side, and is still more confined in a direction at right angles to this. Even in the third direction, to which alone Dr. Hodgson evidently refers, it cannot properly be said that it is infinite in perception. Look at a position in the sky a thousand miles off. Now look at a position a million miles off. What is the difference in perception? Nothing. How then can an infinite distance be perceived when these immensely-unlike finite distances cannot be perceived as differing from one another, or from an infinite distance? Dr. Hodgson has used the wrong word. Instead of saying that Space as perceived is infinite, he should have said that, in perception, Space is finite in two dimensions, and becomes *indefinite* in the third when this becomes great.

I come now to the paragraph beginning "Mr. Spencer then turns to the second or subjective hypothesis, that of Kant." This paragraph is somewhat difficult to deal with, for the reason that in it my reasoning is criticized

both from the Kantian point of view and from Dr. Hodgson's own point of view. Dissenting from Kant's view, Dr. Hodgson says, "I hold that both Space and Time and Feeling, or the material element, are equally and alike subjective, equally and alike objective." As I cannot understand this, I am unable to deal with those arguments against me which Dr. Hodgson bases upon it, and must limit myself to that which he urges on behalf of Kant. He says:—

"But I think that Mr. Spencer's representation of Kant's view is very incorrect; he seems to be misled by the large term non-ego Kant held that Space and Time were *in their origin* subjective, but when applied to the non-ego resulted in phenomena, and were the formal element in those phenomena, among which some were phenomena of the internal sense or ego, others of the external sense or non-ego. The non-ego to which the forms of Space and Time did not apply and did not belong, was the Ding-an-sich, not the phenomenal non-ego. Hence the objective existence of Space and Time in phenomena, but not in the Ding-an-sich, is a consistent and necessary consequence of Kant's view of their subjective origin."

If I have misunderstood Kant, as thus alleged, then my comment must be that I credited him with an hypothesis less objectionable than that which he held. I supposed his view to be that Space, as a form of intuition belonging to the *ego*, is imposed by it on the *non-ego* (by which I understood the thing in itself) in the act of intuition. But now the Kantian doctrine is said to be that Space, originating in the *ego*, when applied to the *non-ego*, results in phenomena (the *non-ego* meant being, in that case, necessarily the Ding-an-sich, or thing in itself); and that the phenomena so resulting become objective existences along with the Space given to them by the subject. The subject having imposed Space as a form on the primordial object, or thing in itself, and so created phenomena, this Space thereupon becomes

an objective existence, independent of both the subject and the original thing in itself! To Dr. Hodgson this may seem a more tenable position than that which I ascribed to Kant; but to me it seems only a multiplication of inconceivabilities. I am content to leave it as it stands: not feeling my reasons for rejecting the Kantian hypothesis much weakened.*

The remaining reply which Dr. Hodgson makes runs thus:—

"But Mr. Spencer has a second argument to prove this inconceivability. It is this:—'If Space and Time are forms of thought, they can never be thought of; since it is impossible for anything to be at once the *form* of thought and the *matter* of thought.' . . . . An instance will show the fallacy best. Syllogism is usually held to be a form of thought. Would it be any argument for the inconceivability of syllogisms to say, they cannot be at once the form and the matter of thought? Can we not syllogize about syllogism? Or, more plainly still,—no dog can bite himself, for it is impossible to be at once the thing that bites and the thing that is bitten."

Had Dr. Hodgson quoted the whole of the passage from which he takes the above sentence; or had he considered it in conjunction with the Kantian doctrine to which it refers (namely, that Space survives in conscious-

* Instead of describing me as misunderstanding Kant on this point, Dr. Hodgson should have described Kant as having, in successive sentences, so changed the meanings of the words he uses, as to make either interpretation possible. At the outset of his *Critique of Pure Reason*, he says:—"The effect of an object upon the faculty of representation, so far as we are affected by the said object, is sensation. That sort of intuition which relates to an object by means of sensation, is called an empirical intuition. The undetermined object of an empirical intuition, is called *phænomenon*. That which in the phænomenon corresponds to the sensation, I term its *matter;*" (here, remembering the definition just given of phenomenon, objective existence is manifestly referred to) "but that which effects that the content of the phænomenon can be arranged under certain relations, I call its *form*" (so that *form*, as here applied, refers

ness when all contents are expelled, which implies that then Space is the thing with which consciousness is occupied, or the *object* of consciousness), he would have seen that his reply has none of the cogency he supposes. If, taking his first illustration, he will ask himself whether it is possible to "syllogize about syllogism," when syllogism has no content whatever, symbolic or other—has nonentity to serve for major, nonentity for minor, and nonentity for conclusion; he will, I think, see that syllogism, considered as surviving terms of every kind, cannot be syllogized about: the "pure form" of reason (supposing it to be syllogism, which it is not) if absolutely discharged of all it contains, cannot be represented in thought, and therefore cannot be reasoned about. Following Dr. Hodgson to his second illustration, I must express my surprise that a metaphysician of his acuteness should have used it. For an illustration to have any value, the relation between the terms of the analogous case must have some parallelism to the relation between the terms of the case with which it is compared. Does Dr. Hodgson really think that the relation between a dog and the part of himself which he bites, is like the relation between *matter* and *form*? Suppose the dog bites his tail. Now the dog, as biting, stands, according to Dr. Hodgson, for the form as the containing mental faculty; and the tail, as bitten, stands for this mental faculty as contained. Now suppose the dog loses his tail. Can the

to objective existence). "But that in which our sensations are merely arranged, and by which they are susceptible of assuming a certain form, cannot be itself sensation." (In which sentence the word *form* obviously refers to subjective existence.) At the outset, the 'phenomenon' and the 'sensation' are distinguished as objective and subjective respectively; and then, in the closing sentences, the *form* is spoken of in connexion first with the one and then with the other, as though they were the same.

faculty as containing and the faculty as contained be separated in the same way? Does the mental form when deprived of all content, even itself (granting that it can be its own content), continue to exist in the same way that a dog continues to exist when he has lost his tail? Even had this illustration been applicable, I should scarcely have expected Dr. Hodgson to remain satisfied with it. I should have thought he would prefer to meet my argument directly, rather than indirectly. Why has he not shown the invalidity of the reasoning used in the *Principles of Psychology* (§ 399, 2nd ed.)? Having there quoted the statement of Kant, that "Space and Time are not merely forms of sensuous intuition, but *intuitions* themselves;" I have written—

"If we inquire more closely, this irreconcilability becomes still clearer. Kant says:—'That which in the phænomenon corresponds to the sensation, I term its *matter*; but that which effects that the content of the phænomenon can be arranged under certain relations, I call its *form*.' Carrying with us this definition of form, as 'that which effects that the content . . . . can be arranged under certain relations,' let us return to the case in which the intuition of Space is the intuition which occupies consciousness. Can the content of this intuition 'be arranged under certain relations' or not? It can be so arranged, or rather, it *is* so arranged. Space cannot be thought of save as having parts, near and remote, in this direction or the other. Hence, if that is the form of a thing 'which effects that the content . . . . can be arranged under certain relations,' it follows that when the content of consciousness is the intuition of Space, which has 'parts that can be arranged under certain relations,' there must be a form of that intuition. What is it? Kant does not tell us—does not appear to perceive that there must be such a form; and could not have perceived this without abandoning his hypothesis that the space-intuition is primordial."

Now when Dr. Hodgson has shown me how that "which effects that the content . . . . can be arranged under certain relations," may also be that which effects its own arrangement under the same relations, I shall

be ready to surrender my position; but until then, no analogy drawn from the ability of a dog to bite himself will weigh much with me.

Having, as he considers, disposed of the reasons given by me for concluding that, considered in themselves, "Space and Time are wholly incomprehensible" (he continually uses on my behalf the word "inconceivable," which, by its unfit connotations, gives a wrong aspect to my position), Dr. Hodgson goes on to say:—

"Yet Mr. Spencer proceeds to use these inconceivable ideas as the basis of his philosophy. For mark, it is Space and Time as we know them, the actual and phenomenal Space and Time, to which all these inconceivabilities attach. Mr. Spencer's result ought, therefore, logically to be—Scepticism. What is his actual result? Ontology And how so? Why, instead of rejecting Space and Time as the inconceivable things he has tried to demonstrate them to be, he substitutes for them an Unknowable, a something which they really are, though we cannot know it, and rejects that, instead of them, from knowledge."

This statement has caused me no little astonishment. That having before him the volume from which he quotes, so competent a reader should have so completely missed the meaning of the passages (§ 26) already referred to, in which I have contended against Hamilton and Mansel, makes me almost despair of being understood by any ordinary reader. In that section I have, in the first place, contended that the consciousness of an Ultimate Reality, though not capable of being made a thought, properly so called, because not capable of being brought within limits, nevertheless remains as a consciousness that is *positive*: is not rendered *negative* by the negations of limits. I have pointed out that—

"The error, (very naturally fallen into by philosophers intent on demonstrating the limits and conditions of consciousness), consists in assuming that consciousness contains *nothing but* limits and conditions; to the entire neglect of that which is limited and con-

ditioned. It is forgotten that there is something which alike forms the raw material of definite thought and remains after the definiteness which thinking gave to it has been destroyed"—something which "ever persists in us as the body of a thought to which we can give no shape."

This *positive* element of consciousness it is which, "at once necessarily indefinite and necessarily indestructible," I regard as the consciousness of the Unknowable Reality. Yet Dr. Hodgson says "Mr. Spencer proceeds to use these inconceivable ideas as the basis of his philosophy:" implying that such basis consists of negations, instead of consisting of that which persists *notwithstanding the negation of limits*. And then, beyond this perversion, or almost inversion, of meaning, he conveys the notion that I take as the basis of philosophy, the "inconceivable ideas" "or self-contradictory notions" which result when we endeavour to comprehend Space and Time. He speaks of me as proposing to evolve substance out of form, or rather, out of the negations of forms—gives his readers no conception that the *Power* manifested to us is that which I regard as the Unknowable, while what we call Space and Time answer to the unknowable *nexus* of its manifestations. And yet the chapter from which I quote, and still more the chapter which follows it, makes this clear—as clear, at least, as I can make it by carefully-worded statements and re-statements.

Philosophical systems, like theological ones, following the law of evolution in general, severally become in course of time more rigid, while becoming more complex and more definite; and they similarly become less alterable—resist all compromise, and have to be replaced by the more plastic systems that descend from them.

It is thus with pure Empiricism and pure Transcendentalism. Down to the present time disciples of Locke

have continued to hold that all mental phenomena are interpretable as results of accumulated individual experiences; and, by criticism, have been led simply to elaborate their interpretations—ignoring the proofs of inadequacy. On the other hand, disciples of Kant, asserting this inadequacy, and led by perception of it to adopt an antagonist theory, having persisted in defending that theory under a form presenting fatal inconsistencies. And then, when there is offered a mode of reconciliation, the spirit of no-compromise is displayed: each side continuing to claim the whole truth. After it has been pointed out that all the obstacles in the way of the experiential doctrine disappear if the effects of ancestral experiences are joined with the effects of individual experiences, the old form of the doctrine is still adhered to. And meanwhile Kantists persist in asserting that the *ego* is born with intuitional forms which are wholly independent of anything in the *non-ego*, after it has been shown that the innateness of these intuitional forms may be so understood as to escape the insurmountable difficulties of the hypothesis as originally expressed.

I am led to say this by reading the remarks concerning my own views, made with an urbanity I hope to imitate, by Professor Max Müller, in a lecture delivered at the Royal Institution last March.* Before dealing with the criticisms contained in this lecture, I must enter a demurrer against that interpretation of my views by which Professor Max Müller makes it appear that they are more allied to those of Kant than to those of Locke. He says:—

"Whether the prehistoric genesis of these congenital dispositions or inherited necessities of thought, as suggested by Mr. Herbert Spencer, be right or wrong, does not signify for the purpose which

* See *Fraser's Magazine* for May last.

Kant had in view. In admitting that there is something in our mind. which is not the result of our own *à posteriori* experience, Mr. Herbert Spencer is a thorough Kantian, and we shall see that he is a Kantian in other respects too. If it could be proved that nervous modifications, accumulated from generation to generation, could result in nervous structures that are fixed in proportion as the outer relations to which they answer are fixed, we, as followers of Kant, should only have to put in the place of Kant's intuitions of Space and Time 'the constant space-relations expressed in definite nervous structures, congenitally framed to act in definite ways, and incapable of acting in any other way.' If Mr. Herbert Spencer had not misunderstood the exact meaning of what Kant calls the intuitions of Space and Time, he would have perceived that, barring his theory of the prehistoric origin of these intuitions, he was quite at one with Kant."

On this passage let me remark, first, that the word "pre-historic," ordinarily employed only in respect to human history, is misleading when applied to the history of Life in general; and his use of it leaves me in some doubt whether Professor Max Müller has rightly conceived the hypothesis he refers to.

My second comment is, that the description of me as "quite at one with Kant," "*barring*" the "theory of the prehistoric origin of these intuitions," curiously implies that it is a matter of comparative indifference whether the forms of thought are held to be *naturally generated* by intercourse between the organism and its environing relations, during the evolution of the lowest into the highest types, or whether such forms are held to be *supernaturally given* to the human mind, and are independent both of environing relations and of ancestral minds. But now, addressing myself to the essential point, I must meet the statement that I have "misunderstood the exact meaning of what Kant calls the intuitions of Space and Time," by saying that I think Professor Max Müller has overlooked certain passages which justify my interpretation, and render his inter-

pretation untenable. For Kant says "Space is *nothing else* than the form of all phenomena of the external sense;" further, he says that "Time is *nothing but* the form of our internal intuition;" and, to repeat words I have used elsewhere, "He distinctly shuts out the supposition that there are forms of the *non-ego* to which these forms of the *ego* correspond, by saying that 'Space is not a conception which has been derived from outward experiences.'" Now so far from being in harmony with, these statements are in direct contradiction to, the view which I hold; and seem to me absolutely irreconcilable with it. How can it be said that, "barring" a difference represented as trivial, I am "quite at one with Kant," when I contend that these subjective forms of intuition are moulded into correspondence with, and therefore derived from, some objective form or *nexus*, and therefore dependent upon it; while the Kantian hypothesis is that these subjective forms are not derived from the object, but pre-exist in the subject—are imposed by the *ego* on the *non-ego*. It seems to me that not only do Kant's words, as above given, exclude the view which I hold, but also that Kant could not consistently have held any such view. Rightly recognizing, as he did, these forms of intuition as innate, he was, from his stand-point, *obliged* to regard them as imposed on the matter of intuition in the act of intuition. In the absence of the hypothesis that intelligence has been evolved, it was *not possible* for him to regard these subjective forms as having been derived from objective forms.

A disciple of Locke might, I think, say that the Evolution-view of our consciousness of Space and Time is essentially Lockian, with more truth than Professor Max Müller can represent it as essentially Kantian. The Evolution-view is completely experiential. It differs

from the original view of the experientialists by containing a great extension of that view. With the relatively-small effects of individual experiences, it joins the relatively-vast effects of the experiences of antecedent individuals. But the view of Kant is avowedly and absolutely un-experiential. Surely this makes the predominance of kinship manifest.

In Professor Max Müller's replies to my criticisms on Kant, I cannot see greater validity than in this affiliation to which I have demurred. One of his arguments is that which Dr. Hodgson has used, and which I have already answered; and I think that the others, when compared with the passages of the *Principles of Psychology* which they concern, will not be found adequate. I refer to them here chiefly for the purpose of pointing out that when he speaks of me as bringing "three arguments against Kant's view," he understates the number. Let me close what I have to say on this disputed question, by quoting the summary of reasons I have given for rejecting the Kantian hypothesis:—

"Kant tells us that Space is the form of all external intuition; which is not true. He tells us that the consciousness of Space continues when the consciousness of all things contained in it is suppressed; which is also not true. From these alleged facts he *infers* that Space is an *à priori* form of intuition. I say *infers*, because this conclusion is not presented in necessary union with the premises, in the same way that the consciousness of duality is necessarily presented along with the consciousness of inequality; but it is a conclusion voluntarily drawn for the purpose of explaining the alleged facts. And then that we may accept this conclusion, which is not necessarily presented along with these alleged facts which are not true, we are obliged to affirm several propositions which cannot be rendered into thought. When Space is itself contemplated, we have to conceive it as at once the form of intuition and the matter of intuition; which is impossible. We have to unite that which we are conscious of as Space with that which we are conscious of as the *ego*, and contemplate the one as a property of the other; which is

impossible. We have at the same time to disunite that which we are conscious of as Space, from that which we are conscious of as the *non-ego*, and contemplate the one as separate from the other; which is also impossible. Further, this hypothesis that Space is "nothing else" than a form of intuition belonging wholly to the *ego*, commits us to one of the two alternatives, that the *non-ego* is formless or that its form produces absolutely no effect upon the *ego*; both of which alternatives involve us in impossibilities of thought." —*Prin. of Psy.*, § 399.

Objections of another, though allied, class have been made in a review of the *Principles of Psychology* by Mr. H. Sidgwick—a critic whose remarks on questions of mental philosophy always deserve respectful consideration.

Mr. Sidgwick's chief aim is to show what he calls "the mazy inconsistency of his [my] metaphysical results." More specifically, he expresses thus the proposition he seeks to justify—"His view of the subject appears to have a fundamental incoherence, which shows itself in various ways on the surface of his exposition, but of which the root lies much deeper, in his inability to harmonise different lines of thought."

Before dealing with the reasons given for this judgment, let me say that, in addition to the value which candid criticisms have as showing where more explanation is needed, they are almost indispensable as revealing to a writer incongruities he had not perceived. Especially where, as in this case, the subject-matter has many aspects, and where the words supplied by our language are so inadequate in number that, to avoid cumbrous circumlocution, they have to be used in senses that vary according to the context, it is extremely difficult to avoid imperfections of statement. But while I acknowledge sundry such imperfections and the resulting incongrui-

ties, I cannot see that these are, as Mr. Sidgwick says, fundamental. Contrariwise, their superficiality seems to me proved by the fact that they may be rectified without otherwise altering the expositions in which they occur. Here is an instance.

Mr. Sidgwick points out that, when treating of the "Data of Psychology," I have said (in § 56) that, though we reach inferentially "the belief that mind and nervous action are the subjective and objective faces of the same thing, we remain utterly incapable of seeing, and even of imagining, how the two are related" (I quote the passage more fully than he does). He then goes on to show that in the "Special Synthesis," where I have sketched the evolution of Intelligence under its objective aspect, as displayed in the processes by which beings of various grades adjust themselves to surrounding actions, I "speak as if" we could see how consciousness "naturally arises at a particular stage" of nervous action. The chapter he here refers to is one describing that "differentiation of the psychical from the physical life" which accompanies advancing organization, and more especially advancing development of the nervous system. In it I have shown that, while the changes constituting physical life continue to be characterized by the *simultaneity* with which all kinds of them go on throughout the organism, the changes constituting psychical life, arising as the nervous system develops, become gradually more distinguished by their *seriality*. And I have said that as nervous integration advances, "there must result an unbroken series of these changes—there must arise a consciousness." Now I admit that here is an apparent inconsistency. I ought to have said that "there must result an unbroken series of these changes," which, taking place in the nervous system of a highly-organized

creature, gives coherence to its conduct; and along with which we assume a consciousness, because consciousness goes along with coherent conduct in ourselves. If Mr. Sidgwick will substitute this statement for the statement as it stands, he will see that the arguments and conclusions remain intact. A survey of the chapter as a whole, proves that its aim is not in the least to explain how nervous changes, considered as waves of molecular motion, become the feelings constituting consciousness; but that, contemplating the facts objectively in living creatures at large, it points out the cardinal distinction between vital actions in general, and those particular vital actions which, in a creature displaying them, lead us to speak of it as intelligent. It is shown that the rise of such actions becomes marked in proportion as the changes taking place in the part called the nervous system, are made more and more distinctly serial, by union in a supreme centre of co-ordination. The introduction of the word consciousness, arises in the effort to show what fundamental character there is in these particular physiological changes which is *parallel to* a fundamental character in the psychological changes.

Another instance of the way in which Mr. Sidgwick evolves an incongruity which he considers fundamental, out of what I should have thought he would see is a defective expression, I will give in his own words. Speaking of a certain view of mine, he says:—

"He tells us that 'logic . . . contemplates in its propositions certain connexions predicated, which are necessarily involved with certain other connexions given: *regarding all these connexions as existing in the non-ego*—not, it may be, under the form in which we know them, but in some form.' But in § 473, where Mr. Spencer illustrates by a diagram his 'Transfigured Realism', the view seems to be this: although we cannot say that the real non-ego resembles our notion of it in 'its elements, relations, or laws,' we can say that

'a change in the objective reality causes in the subjective state a change exactly answering to it—so answering as to *constitute a cognition of it.*' Here the 'something beyond consciousness' is no longer said to be unknown, as its effect in consciousness 'constitutes a cognition of it.'"

This apparent inconsistency, marked by the italics, would not have existed if, instead of "a cognition of it," I had said, as I ought to have said, "*what we call* a cognition of it"—that is, a relative cognition as distinguished from an absolute cognition. In ordinary language we speak of as cognitions, those connexions in thought which so guide us in our dealings with things, that actual experience verifies ideal anticipation: marking off, by opposed words, those connexions in thought which *mis*-guide us. The difference between accepting a cognition as relatively-true and accepting it as absolutely-true, will be clearly shown by an illustration. There is no direct resemblance whatever between the sizes, forms, colours, and arrangements, of the figures in an account-book, and the moneys or goods, debts or credits, represented by them; and yet the forms and arrangements of the written symbols, are such as answer in a perfectly-exact way to stocks of various commodities and to various kinds of transactions. Hence we say, figuratively, that the account-book will "tell us" all about these stocks and transactions. Similarly, the diagram Mr. Sidgwick refers to, suggests a way in which symbols, registered in us by objects, may have forms and arrangements wholly unlike their objective causes and the *nexus* among those causes, while yet they are so related as to guide us correctly in our transactions with those objective causes, and, *in that sense*, constitute cognitions of them; though they no more constitute cognitions in the absolute sense, than do the guiding symbols in the account-book constitute cognitions of the things to which they refer. So

repeatedly is this view implied throughout the *Principles of Psychology,* that I am surprised to find a laxity of expression raising the suspicion that I entertain any other.

To follow Mr. Sidgwick through sundry criticisms of like kind, which may be similarly met, would take more space than I can here afford. I must restrict myself now to the alleged "fundamental incoherence" of which he thinks these inconsistencies are signs. I refer to that reconciliation of Realism and Idealism considered by him as an impossible compromise. A difficulty is habitually felt in accepting a coalition after long conflict. Whoever has espoused one of two antagonist views, and, in defending it, has gained a certain comprehension of the opposite view, becomes accustomed to regard these as the only alternatives, and is puzzled by an hypothesis which is at once both and neither. Yet, since it turns out in nearly all cases that, of conflicting doctrines, each contains an element of truth, and that controversy ends by combination of their respective half-truths, there is *à priori* probability on the side of an hypothesis which qualifies Realism by Idealism.

Mr. Sidgwick expresses his astonishment, or rather bespeaks that of his readers, because, while I accept Idealistic criticisms, I nevertheless defend the fundamental intuition of Common Sense; and, as he puts it, "fires his [my] argument full in the face of Kant, Mill, and 'metaphysicians' generally."

"He tells us that 'metaphysicians' illegitimately assume that 'beliefs reached through complex intellectual processes,' are more valid than 'beliefs reached through simple intellectual processes;' that the common language they use refuses to express their hypotheses, and thus their reasoning inevitably implies the common notions which they repudiate; that the belief of Realism has the advantage of 'priority,' 'simplicity,' 'distinctness.' But surely

this prior, simple, distinctly affirmed belief is that of what Mr. Spencer terms 'crude Realism,' the belief that the non-ego is *per se* extended, solid, even coloured (if not resonant and odorous). This is what common language implies; and the argument by which Mr. Spencer proves the relativity of feelings and relations, still more the subtle and complicated analysis by which he resolves our notion of extension into an aggregate of feelings and transitions of feeling, lead us away from our original simple belief—that (*e.g.*) the green grass we see exists out of consciousness as we see it—just as much as the reasonings of Idealism, Scepticism, or Kantism."

On the face of it the anomaly seems great; but I should have thought that after reading the chapter on "Transfigured Realism," a critic of Mr. Sidgwick's acuteness would have seen the solution of it. He has overlooked an essential distinction. All which my argument implies is that the direct intuition of Realism must be held of superior authority to the arguments of Anti-Realism, *where their deliverances cannot be reconciled.* The one point on which their deliverances cannot be reconciled, is the existence of an objective reality. But while against this intuition of Realism I hold the arguments of Anti-Realism to be powerless, because they cannot be carried on without postulating that which they end by denying; yet, having admitted objective existence as a necessary postulate, it is possible to make valid criticisms upon all those judgments which Crude Realism joins with this primordial judgment: it is possible to show that a transfigured interpretation of properties and relations, is more tenable than the original interpretation.

To elucidate the matter, let us take the most familiar case in which the indirect judgments of Reason correct the direct judgments of Common Sense. The direct judgment of Common Sense is that the Sun moves round the Earth. In course of time, Reason, finding

some facts at variance with this, begins to doubt; and, eventually, hits upon an hypothesis which explains the anomalies, but which denies this apparently-certain *dictum* of Common Sense. What is the reconciliation? It consists in showing to Common Sense that the new interpretation equally well corresponds with direct intuition, while it avoids all the difficulties. Common Sense is reminded that the apparent motion of an object may be due either to its actual motion or to the motion of the observer; and that there are terrestrial experiences in which the observer thinks an object he looks at is moving, when the motion is in himself. Extending the conception thus given, Reason shows that if the Earth revolves on its axis, there will result that apparent motion of the Sun which Common Sense interpreted into an actual motion of the Sun; and the common-sense observer thereupon becomes able to think of sunrise and sunset as due to his position as spectator on a vast revolving globe. Now if the astronomer, setting out by recognizing these celestial appearances, and proceeding to evolve the various anomalies following from the common-sense interpretation of them, had drawn the conclusion that there externally exist no Sun and no motion at all, he would have done what Idealists do; and his arguments would have been equally powerless against the intuition of Common Sense. But he does nothing of the kind. He accepts the intuition of Common Sense respecting the reality of the Sun and of the motion; but replaces the old interpretation of the motion by a new interpretation reconcilable with all the facts.

Everyone must see that here, acceptance of the inexpugnable element in the common-sense judgment, by no means involves acceptance of the accompanying judgments; and I contend that the like discrimination must

be made in the case we are considering. It does not follow that while, against the consciousness which Crude Realism has of an objective reality, the arguments of Anti-Realism are futile, they are therefore futile against the conceptions which Crude Realism forms of the objective reality. If Anti-Realism can show that, granting an objective reality, the interpretation of Crude Realism contains insuperable difficulties, the process is quite legitimate. And, its primordial intuition remaining unshaken, Realism may, on reconsideration, be enabled to frame a new conception which harmonizes all the facts.

To show that there is not here the "mazy inconsistency" alleged, let us take the case of sound as interpreted by Crude Realism, and as re-interpreted by Transfigured Realism. Crude Realism assumes the sound present in consciousness to exist as such beyond consciousness. Anti-Realism proves the inadmissibility of this assumption in sundry ways (all of which, however, set out by talking of sounding bodies beyond consciousness, just as Realism talks of them); and then Anti-Realism concludes that we know of no existence save the sound as a mode of consciousness: which conclusion, and all kindred conclusions, I contend are vicious—first, because all the words used connote an objective activity; second, because the arguments are impossible without postulating at the outset an objective activity; and third, because no one of the intuitions out of which the arguments are built, is of equal validity with the single intuition of Realism that an objective activity exists. But now the Transfigured Realism which Mr. Sidgwick thinks "has all the serious incongruity of an intense metaphysical dream," neither affirms the untenable conception of Crude Realism, nor, like Anti-Realism, draws unthinkable conclusions by suicidal arguments; but,

accepting that which is essential in Crude Realism, and admitting the difficulties which Anti-Realism insists upon, reconciles matters by a re-interpretation analogous to that which an astronomer makes of the solar motion. Continuing all along to recognize an objective activity which Crude Realism calls sound, it shows that the answering sensation is produced by a succession of separate impacts which, if made slowly, may be separately identified, and which will, if progressively increased in rapidity, produce tones higher and higher in pitch. It shows by other experiments that sounding bodies are in states of vibration, and that the vibrations may be made visible. And it concludes that the objective activity is not what it subjectively seems, but is proximately interpretable as a succession of aërial waves. Thus Crude Realism is shown that while there unquestionably exists an objective activity corresponding to the sensation known as sound, yet the facts are not explicable on the original supposition that this is like the sensation; while they are explicable by conceiving it as a rhythmical mechanical action. Eventually this re-interpretation, joined with kindred re-interpretations of other sensations, comes to be itself further transfigured by analysis of its terms, and re-expression of them in terms of molecular motion; but, however abstract the interpretation ultimately reached, the objective activity continues to be postulated: the primordial judgment of Crude Realism remains unchanged, though it has to change the rest of its judgments.

In another part of his argument, however, Mr. Sidgwick implies that I have no right to use those conceptions of objective existence by which this compromise is effected. Quoting sundry passages to show that while I hold the criticisms of the Idealist to be impossible without "tacitly

or avowedly postulating an unknown something beyond consciousness," I yet admit that "our states of consciousness are the only things we can know;" he goes on to argue that I am radically inconsistent, because, in interpreting the phenomena of consciousness, I continually postulate, not an unknown something, but a something of which I speak in ordinary terms, as though its ascribed physical characters really exist as such, instead of being, as I admit they are, synthetic states of my consciousness. His objection, if I understand it, is that for the purposes of Objective Psychology I apparently profess to know Matter and Motion in the ordinary realistic way; while, as a result of subjective analysis, I reach the conclusion that it is impossible to have that knowledge of objective existence which Realism supposes we have. Doubtless there seems here to be what he calls "a fundamental incoherence." But I think it exists, not between my two expositions, but between the two consciousnesses of subjective and objective existence, which we cannot suppress and yet cannot put into definite forms. The alleged incoherence I take to be but another name for the inscrutability of the relation between subjective feeling and its objective correlate which is not feeling—an inscrutability which meets us at the bottom of all our analyses. An exposition of this inscrutability I have elsewhere summed up thus:—

"See, then, our predicament. We can think of Matter only in terms of Mind. We can think of Mind only in terms of Matter. When we have pushed our explorations of the first to the uttermost limit, we are referred to the second for a final answer; and when we have got the final answer of the second, we are referred back to the first for an interpretation of it. We find the value of x in terms of y; then we find the value of y in terms of x; and so on we may continue for ever without coming nearer to a solution."—*Prin. of Psy.* § 272.

Carrying a little further this simile, will, I think, show where lies the insuperable difficulty felt by Mr Sidgwick.

Taking x and y as the subjective and objective activities, unknown in their natures and known only as phenomenally manifested; and recognizing the fact that every state of consciousness implies, immediately or remotely, the action of object on subject or subject on object, or both; we way say that every state of consciousness will be symbolized by some modification of xy—the phenomenally-known product of the two unknown factors. In other words, xy', $x'y$, $x'y'$, $x''y'$, $x'y''$, &c., &c., will represent all perceptions and thoughts. Suppose, now, that these are thoughts about the object; composing some hypothesis respecting its characters as analyzed by physicists. Clearly, all such thoughts, be they about shapes, resistances, momenta, molecules, molecular motions, or what not, will contain forms of the subjective activity x. Now let the thoughts be concerning mental processes. It must similarly happen that some mode of the unknown objective activity y, will be in every case a component. Now suppose that the problem is the genesis of mental phenemena; and that in the course of the inquiry, bodily organization and the functions of the nervous system are brought into the explanation. It will happen, as before, that these, considered as objective, have to be described and thought about in modes of xy. And when by the actions of such a nervous system, conceived objectively in modes of xy, and acted upon by physical forces which are conceived in other modes of xy, we endeavour to explain the genesis of sensations, perceptions, and ideas, which we can think of only in other modes of xy, we find that all our factors, and therefore all our interpretations, contain the two unknown terms, and that no interpretation is imaginable that will not contain the two unknown terms.

What is the defence for this apparently-circular process? Simply that it is a process of establishing

congruity among our symbols. It is finding a mode of so symbolizing the unknown activities, subjective and objective, and so operating with our symbols, that all our acts may be rightly guided—guided, that is, in such ways that we can anticipate, when, where, and in what quantity some one of our symbols, or some combination of our symbols, will be found. Mr. Sidgwick's difficulty arises, I think, from having insufficiently borne in mind the statements made at the outset, in "The Data of Philosophy," that such conceptions as "are vital, or cannot be separated from the rest without mental dissolution, must be assumed as true *provisionally;*" that "there is no mode of establishing the validity of any belief except that of showing its entire *congruity* with all other beliefs;" and that "Philosophy, compelled to make those fundamental assumptions without which thought is impossible, has to justify them by showing their *congruity* with all other dicta of consciousness." In pursuance of this distinctly-avowed mode of procedure, I assume provisionally, an objective activity and a subjective activity, and certain general forms and modes (Space, Time, Matter, Motion, Force,) which the subjective activity, operated on by the objective activity, ascribes to it, and which I suppose to correspond in some way to unknown forms and modes of the objective activity. These provisional assumptions, having been carried out to all their consequences, and these consequences proved to be congruous with one another and with the original assumptions, these original assumptions are justified. And if, finally, I assert, as I have repeatedly asserted, that the terms in which I express my assumptions and carry on my operations are but symbolic, and that all I have done is to show that by certain ways of symbolizing, perfect harmony results—

invariable agreement between the symbols in which I frame my expectations, and the symbols which occur in experience—I cannot be blamed for incoherence. On the contrary, it seems to me that my method is the most coherent that can be devised. Lastly, should it be said that this regarding of everything constituting experience and thought as symbolic, has a very shadowy aspect; I reply that these which I speak of as symbols, are real relatively to our consciousness; and are symbolic only in their relation to the Ultimate Reality.

That these explanations will make clear the coherence of views which before seemed "fundamentally incoherent," I feel by no means certain; since, as I did not perceive the difficulties presented by the exposition as at first made, I may similarly fail to perceive the difficulties in this explanation. Originally, I had intended to complete the *Principles of Psychology* by a division showing how the results reached in the preceding divisions, physiological and psychological, analytic and synthetic, subjective and objective, harmonize with one another, and are but different aspects of the same aggregate of phenomena. But the work was already bulky; and I concluded that this division might be dispensed with, because the congruities to be pointed out were sufficiently obvious. So little was I conscious of the alleged "inability to harmonize different lines of thought." Mr. Sidgwick's perplexities, however, show me that such an exposition of concords is needful.

I have reserved to the last, one of the first objections made to the metaphysico-theological doctrine set forth in *First Principles*, and implied in the several volumes that have succeeded it. It was urged by an able metaphysician, the Rev. James Martineau, in an essay entitled

"Science, Nescience, and Faith;" and, effective against my argument as it stands, shows the need for some development of my argument. That Mr. Martineau's criticism may be understood, I must quote the passages it concerns. Continuing the reasoning employed against Hamilton and Mansel, to show that our consciousness of that which transcends knowledge is *positive*, and not, as they allege, *negative*, I have said :—

"Still more manifest will this truth become when it is observed that our conception of the Relative itself disappears, if our conception of the Absolute is a pure negation. It is admitted, or rather it is contended, by the writers I have quoted above, that contradictories can be known only in relation to each other—that Equality, for instance, is unthinkable apart from its correlative Inequality; and that thus the Relative can itself be conceived only by opposition to the Non-relative. It is also admitted, or rather contended, that the consciousness of a relation implies a consciousness of both the related members. If we are required to conceive the relation between the Relative and Non-relative without being conscious of both, 'we are in fact' (to quote the words of Mr. Mansel differently applied) 'required to compare that of which we are conscious with that of which we are not conscious; the comparison itself being an act of consciousness, and only possible through the consciousness of both its objects.' What, then, becomes of the assertion that 'the Absolute is conceived merely by a negation of conceivability,' or as 'the mere absence of the conditions under which consciousness is possible?' If the Non-relative or Absolute, is present in thought only as a mere negation, then the relation between it and the Relative becomes unthinkable, because one of the terms of the relation is absent from consciousness. And if this relation is unthinkable, then is the Relative itself unthinkable, for want of its antithesis: whence results the disappearance of all thought whatever."—*First Principles*, § 26.

On this argument Mr. Martineau comments as follows; first re-stating it in other words :—

"Take away its antithetic term, and the relative, thrown into isolation, is set up as absolute, and disappears from thought. It is indispensable therefore to uphold the Absolute in existence, as con-

dition of the relative sphere which constitutes our whole intellectual domain. Be it so: but, when saved on this plea,—to preserve the balance and interdependence of two *co*-relatives,—the 'Absolute' is absolute no more; it is reduced to a term of relation: it loses therefore its exile from thought: its disqualification is cancelled: and the alleged nescience is discharged.

"So, the same law of thought which warrants the existence, dissolves the inscrutableness, of the Absolute."—*Essays, Philosophical and Theological*, pp. 186-7.

I admit this to be a telling rejoinder; and one which can be met only when the meanings of the words, as I have used them, are carefully discriminated, and the implications of the doctrine fully traced out. We will begin by clearing the ground of minor misconceptions.

First, let it be observed that though I have used the word Absolute as the equivalent of Non-relative, because it is used in the passages quoted from the writers I am contending against; yet I have myself chosen for the purposes of my argument, the name Non-relative, and I do not necessarily commit myself to any propositions respecting the Absolute, considered as that which includes both Subject and Object. The Non-relative as spoken of by me, is to be understood rather as the totality of Being *minus* that which constitutes the individual consciousness, present to us under forms of Relation. Did I use the word in some Hegelian sense, as comprehensive of that which thinks and that which is thought about, and did I propose to treat of the order of things, not as phenomenally manifested but as noumenally proceeding, the objection would be fatal. But the aim being simply to formulate the order of things as present under relative forms, the antithetical Non-relative here named as implied by the conception of the Relative, is that which, in any act of thought, is outside of and beyond it, rather than that which is inclusive of it. Further, it should be

observed that this Non-relative, spoken of as a necessary complement to the Relative, is not spoken of as a conception but as a *consciousness;* and I have in sundry passages distinguished between those modes of consciousness which, having limits, and constituting thought proper, are subject to the laws of thought, and the mode of consciousness which persists when the removal of limits is carried to the uttermost, and when distinct thought consequently ceases.

This opens the way to the reply here to be made to Mr. Martineau's criticism—namely, that while by the necessities of thought the Relative implies a Non-relative; and while, to think of this antithesis completely, requires that the Non-relative shall be made a conception proper; yet, for the vague thought which is alone in this case possible, it suffices that the Non-relative shall be present as a consciousness which though undefined is positive. Let us observe what necessarily happens when thought is employed on this ultimate question.

In a preceding part of the argument criticized, I have, in various ways, aimed to show that, alike when we analyze the product of thought and when we analyze the process of thought, we are brought to the conclusion that invariably "a thought involves *relation, difference, likeness;*" and that even from the very nature of Life itself, we may evolve the conclusion that "thinking being relationing, no thought can ever express more than relations." What, now, must happen if thought, having this law, occupies itself with the final mystery? Always implying terms in relation, thought implies that both terms shall be more or less defined; and as fast as one of them becomes indefinite, the relation also becomes indefinite, and thought becomes indistinct. Take the case of magnitudes. I think of an inch; I think of a

foot; and having tolerably-definite ideas of the two, I have a tolerably-definite idea of the relation between them. I substitute for the foot a mile; and being able to represent a mile much less definitely, I cannot so definitely think of the relation between an inch and a mile—cannot distinguish it in thought from the relation between an inch and two miles, as clearly as I can distinguish in thought the relation between an inch and one foot from the relation between an inch and two feet. And now if I endeavour to think of the relation betwen an inch and the 240,000 miles from here to the Moon, or the relation between an inch and the 92,000,000 miles from here to the Sun, I find that while these distances, practically inconceivable, have become little more than numbers to which I frame no answering ideas, so, too, has the relation between an inch and either of them become practically inconceivable. Now this partial failure in the process of forming thought-relations, which happens even with finite magnitudes when one of them is immense, passes into complete failure when one of them cannot be brought within any limits. The relation itself becomes unrepresentable at the same time that one of its terms becomes unrepresentable. Nevertheless, in this case it is to be observed that the almost-blank form of relation preserves a certain qualitative character. It is still distinguishable as belonging to the consciousness of extensions, not to the consciousnesses of forces or durations; and in so far remains a vaguely-identifiable relation. But now suppose we ask what happens when one term of the relation has not simply magnitude having no known limits, and duration of which neither beginning nor end is cognizable, but is also an existence not to be defined? In other words, what must happen if one term of the relation is

not only quantitatively but also qualitatively unrepresentable? Clearly in this case the relation does not simply cease to be thinkable except as a relation of a certain class, but it lapses completely. When one of the terms becomes wholly unknowable, the law of thought can no longer be conformed to; both because one term cannot be present, and because relation itself cannot be framed. That is to say, the law of thought that contradictories can be known only in relation to each other, no longer holds when thought attempts to transcend the Relative; and yet, when it attempts to transcend the Relative, it must make the attempt in conformity with its law—must in some dim mode of consciousness posit a Non-relative, and, in some similarly dim mode of consciousness, a relation between it and the Relative. In brief then, to Mr. Martineau's objection I reply, that the insoluble difficulties he indicates arise here, as elsewhere, when thought is applied to that which transcends the sphere of thought; and that just as when we try to pass beyond phenomenal manifestations to the Ultimate Reality manifested, we have to symbolize it out of such materials as the phenomenal manifestations give us; so we have simultaneously to symbolize the connexion between this Ultimate Reality and its manifestations, as somehow allied to the connexions among the phenomenal manifestations themselves. The truth Mr. Martineau's criticism adumbrates, is that the law of thought fails where the elements of thought fail; and this is a conclusion quite conformable to the general view I defend. Still holding the validity of my argument against Hamilton and Mansel, that in pursuance of their own principle the Relative is not at all thinkable *as such*, unless in contradistinction to some existence posited, however vaguely, as the other term of a relation, conceived however inde-

finitely; it is consistent on my part to hold that in this effort which thought inevitably makes to pass beyond its sphere, not only does the product of thought become a dim symbol of a product, but the process of thought becomes a dim symbol of a process; and hence any predicament inferable from the law of thought cannot be asserted.

I may fitly close this reply by a counter-criticism. To the direct defence of a proposition, may be added the indirect defence that results from showing the untenability of an alternative proposition. This criticism on the doctrine of an Unknowable Existence manifested to us in phenomena, Mr. Martineau makes in the interests of the doctrine held by him, that this existence is, to a considerable degree, knowable. We are quite at one in holding that there is an indestructible consciousness of Power behind Appearance; but whereas I contend that this Power cannot be brought within the forms of thought, Mr. Martineau contends that there can be consistently ascribed certain attributes of personality—not, indeed, human characteristics so concrete as were ascribed in past times; but still, human characteristics of the more abstract and higher class. His general doctrine is this:—Regarding Matter as independently existing; regarding as also independently existing, those primary qualities of Body "which are inseparable from the very idea of Body, and may be evolved *à priori* from the consideration of it as solid extension or extended solidity;" and saying that to this class "belong Triple Dimension, Divisibility, Incompressibility;" he goes on to assert that as these—

"cannot absent themselves from Body, they have a reality coeval with it, and belong eternally to the material datum objective to God: and his mode of activity with regard to them must be similar to that which alone we can think of his directing upon the relations of Space, viz. not Volitional, to cause them, but Intellectual, to think them out.

The Secondary Qualities, on the other hand, having no logical tie to the Primary, but being appended to them as contingent facts, cannot be referred to any deductive thought, but remain over as products of pure Inventive Reason and Determining Will. This sphere of cognition, *à posteriori* to us,—where we cannot move a step alone but have submissively to wait upon experience, is precisely the realm of Divine originality: and we are most sequacious where He is most free. While on this Secondary field His Mind and ours are thus contrasted, they meet in resemblance again upon the Primary: for the evolutions of deductive Reason there is but one track possible to all intelligences; no *merum arbitrium* can interchange the false and true, or make more than one geometry, one scheme of pure Physics, for all worlds: and the Omnipotent Architect Himself, in realizing the Kosmical conception, in shaping the orbits out of immensity and determining seasons out of eternity, could but follow the laws of curvature, measure, and proportion."—*Essays, Philosophical and Theological*, pp. 163-4.

Before the major criticism which I propose to make on this hypothesis, let me make a minor one. Not only of space-relations, but also of primary physical properties, Mr. Martineau asserts the necessity: not a necessity to our minds simply, but an ontological necessity. What is true for human thought, is, in respect of these, true absolutely: "the laws of curvature, measure, and proportion," as we know them, are unchangeable even by Divine power; as are also the Divisibility and Incompressibility of Matter. But if, in these cases, Mr. Martineau holds that a necessity in thought implies an answering necessity in things, why does he refrain from saying the like in other cases? Why, if he tacitly asserts it in respect of space-relations and the statical attributes of Body, does he not also assert it in respect of the dynamical attributes of Body? The laws conformed to by that mode of force now distinguished as "energy," are as much necessary to our thought as are the laws of space-relations. The axioms of Mechanics lie on the

same plane with the axioms of pure Mathematics. Now if Mr. Martineau admits this—if he admits, as he must, the corollary that there can be no such manifestation of energy as that displayed in the motion of a planet, save at the expense of equivalent energy which pre-existed—if he draws the further necessary corollary that the direction of a motion cannot be changed by any action, without an equal reaction in an opposite direction on something acting—if he bears in mind that this holds not only of all visible motions, celestial and terrestrial, but that those activities of Body which affect us as secondary properties, are also known only through other forms of energy, which are equivalents of mechanical energy and conform to these same laws—and if, lastly, he infers that none of these derivative energies can have given to them their characters and directions, save by pre-existing forces, statical and dynamical, conditioned in special ways; what becomes of that "realm of Divine originality" which Mr. Martineau describes as remaining within the realm of necessity? Consistently carried out, his argument implies a universally-inevitable order, in which volition can have no such place as that he alleges.

Not pushing Mr. Martineau's reasoning to this conclusion, so entirely at variance with the one he draws, but accepting his statement just as it stands, let us consider the solution it offers us. We are left by it without any explanation of Space and Time; we are not helped in conceiving the origin of Matter; and there is afforded us no idea how Matter came to have its primary attributes. All these are tacitly assumed to exist uncreated. Creative activity is represented as under the restrictions imposed by mathematical necessities, and as having for *datum* (mark the word) a substance which, in respect of certain characters, defies modification. But surely this is

not an interpretation of the mystery of things. The mystery is simply relegated to a remoter region, respecting which no inquiry is to be made. But the inquiry *must* be made. After every such solution there arises afresh the question—what is the origin and nature of that which imposes these limits on creative power? what is the primary God which dominates over this secondary God? For, clearly, if the "Omnipotent Architect himself" (to use Mr. Martineau's somewhat inconsistent name) is powerless to change the "material datum objective" to him, and powerless to change the conditions under which it exists, and under which he works, there is obviously implied a power to which he is subject. So that in Mr. Martineau's doctrine also, there is an Ultimate Unknowable; and it differs from the doctrine he opposes, only by intercalating a partially Knowable between this and the wholly Knowable.

Finding, as explained above, that this interpretation is not consistent with itself; and finding, as just shown, that it leaves the essential mystery unsolved; I do not see that it has an advantage over the doctrine of the Unknowable in its unqualified shape. There cannot, I think, be more than temporary rest in a proximate solution which takes for its basis the ultimately insoluble. Just as thought cannot be prevented from passing beyond Appearance, and trying to conceive the Cause behind; so, following out the interpretation Mr. Martineau offers, thought cannot be prevented from asking what Cause it is which restricts the Cause he assigns. And if we must admit that the question under this eventual form cannot be answered, may we not as well confess that the question under its immediate form cannot be answered? Is it not better candidly to acknowledge the incompetence of our intelligence, rather than to persist in calling that

an explanation which does but disguise the inexplicable? Whatever answer each may give to this question, he cannot rightly blame those who, finding in themselves an indestructible consciousness of an ultimate Cause, whence proceed alike what we call the Material Universe and what we call Mind, refrain from affirming anything respecting it; because they find it as inscrutable in nature as it is inconceivable in extent and duration.

Postscript.—With the concluding paragraph of the foregoing article, I had hoped to end, for a long time, all controversial writing; and, if the article had been published entire in the November number of the *Fortnightly*, as originally intended, the need for any addition would not have been pressing. But while it was in the printer's hands, two criticisms, more elaborate than those dealt with above, made their appearance; and now that the postponed publication of this latter half of the article affords the opportunity, I cannot, without risking misinterpretations, leave these criticisms unnoticed.

Especially do I feel called upon by courtesy to make some response to one who, in the *Quarterly Review* for October, has dealt with me in a spirit which, though largely antagonistic, is not wholly unsympathetic; and who manifestly aims to estimate justly the views he opposes. In the space at my disposal, I cannot of course follow him through all the objections he has urged. I must content myself with brief comments on the two propositions he undertakes to establish. His enunciation of these runs thus:—

"We would especially direct attention to two points, to both of which we are confident objections may be made; and although Mr. Spencer has himself doubtless considered such objections (and they

may well have struck many of his readers also), we nevertheless do not observe that he has anywhere noticed or provided for them.

"The two points we so select are:—

"(1) *That his system involves the denial of all truth.*

"(2) *That it is radically and necessarily opposed to all sound principles of morals.*"

On this passage, ending in these two startling assertions, let me first remark that I am wholly without this consciousness the reviewer ascribes to me. Remembering that I have expended some little labour in developing what I conceive to be a system of truths, I am surprised by the supposition that "the denial of all truth" is an implication which I am "doubtless" aware may be alleged against this system. Remembering, too, that by its programme this system is shown to close with two volumes on *The Principles of Morality*, the statement that it is "necessarily opposed to all sound principles of morals," naturally astonishes me; and still more the statement that I am doubtless conscious it may be so regarded. Saying thus much by way of repudiating that latent scepticism attributed to me by the reviewer, I proceed to consider what he says in proof of these propositions.

On those seeming incongruities of Transfigured Realism commented on by him, I need say no more than I have already said in reply to Mr. Sidgwick; by whom also they have been alleged. I will limit myself to the corollary he draws from the doctrine of the Relativity of Knowledge, as held by me. Rightly pointing out that I hold this in common with "Messrs. Mill, Lewes, Bain, and Huxley;" but not adding, as he should have done, that I hold it in common with Hamilton, Mansel, and the long list of predecessors through whom Hamilton traced it; the reviewer proceeds to infer from this doctrine of relativity that no absolute truth of any kind can be asserted—

not even the absolute truth of the doctrine of relativity itself. And then he leaves it to be supposed by his readers, that this inference tells especially against the system he is criticizing. If, however, the reviewer's inference is valid, this "denial of all truth" must be charged against the doctrines of thinkers called orthodox, as well as against the doctrines of those many philosophers, from Aristotle down to Kant, who have said the same thing. But now I go further, and reply that against that form of the doctrine of relativity held by me, this allegation cannot be made with the same effect as it can against preceding forms of the doctrine. For I diverge from other relativists in asserting that the existence of a non-relative is not only a positive deliverance of consciousness, but a deliverance transcending in certainty all others whatever; and is one without which the doctrine of relativity cannot be framed in thought. I have urged that "unless a real Non-relative or Absolute be postulated, the Relative itself becomes absolute; and so brings the argument to a contradiction;"* and elsewhere I have described this consciousness of a Non-relative manifested to us through the Relative as "deeper than demonstration—deeper even than definite cognition—deep as the very nature of mind;"† which seems to me to be saying as emphatically as possible that, while all other truths may be held as relative, this truth must be held as absolute. Yet, strangely enough, though contending thus against the pure relativists, and holding with the reviewer, that "every asserter of such a [purely-relative] philosophy must be in the position of a man who saws across the branch of a tree on which he actually sits, at a point between himself and the trunk,"‡ I am

* *First Principles*, § 26. † *Ibid.*, § 62.

‡ Compare *Principles of Psychology*, §§ 88, 95, 391, 401, 406.

singled out by him as though this were my own predicament! So far, then, from admitting that the view I hold "involves the denial of all truth," I assert that, having at the outset posited the co-existence of subject and object as a deliverance of consciousness which precedes all reasoning;* having subsequently shown, analytically, that this postulate is in every way verified,† and that in its absence the proof of relativity is impossible; my view is distinguished by an exactly-opposite trait.

The justification of his second proposition the reviewer commences by saying that—"In the first place the process of Evolution, as understood by Mr. Spencer, compels him to be at one with Mr. Darwin in his denial of the existence of any fundamental and essential distinction between Duty and Pleasure." Following this by a statement respecting the genesis of moral sentiments as understood by me (which is extremely unlike the one I have given in the *Principles of Psychology*, § 215, §§ 503-512, and §§ 524-532), the reviewer goes on to say that "We yield with much reluctance to the necessity of affirming that Mr. Spencer gives no evidence of ever having acquired a knowledge of the meaning of the term 'morality,' according to the true sense of the word."

Just noting that, as shown by the context, the assertion thus made is made against all those who hold the Doctrine of Evolution in its unqualified form, I reply that in so far as it concerns me, it is one the reviewer would scarcely have made had he more carefully examined the evidence: not limiting himself to those works of mine named at the head of his article. And I cannot but think that had the spirit of fairness which he evidently strives to maintain, been fully awake when these

* *First Principles*, §§ 39—45. † *Principles of Psychology*, part vii.

passages were written, he would have seen that, before making so serious an allegation, wider inquiry was needful. If he had simply said that, given the doctrine of mental evolution as held by me, he failed to see how moral principles are to be established, I should not have objected; provided he had also said that I believe they can be established, and had pointed out what I hold to be their bases. As it is, however, he has so presented his own inference from my premises, as to make it seem an inference which I also must draw from my premises. Quite a different and much more secure foundation for moral principles is alleged by me, than that afforded by moral sentiments and conceptions; which he refers to as though they formed the sole basis of the ethical conclusions I hold. While the reviewer contends that "Mr. Spencer's moral system is even yet more profoundly defective, as it denies any objective distinction between right and wrong in any being, whether men are or are not responsible for their actions;" I contend, contrariwise, that it is distinguished from other moral systems by asserting the objectivity of the distinction, and by endeavouring to show that the subjective distinction is derived from the objective distinction. In my first work, *Social Statics*, published twenty-three years ago, the essential thesis is that, apart from their warrant as alleged Divine injunctions, and apart from their authority as moral intuitions, the principles of justice are primarily deducible from the laws of life, as carried on under social conditions. I argued throughout that these principles so derived have a supreme authority, to which considerations of immediate expediency must yield; and I was for this reason classed by Mr. Mill as an anti-utilitarian. More recently, in a letter drawn from me by this misapprehension of Mr. Mill, and afterwards published by Professor

Bain in his *Mental and Moral Science,* I have re-stated this position. Already, in an explanatory article entitled *Morals and Moral Sentiments,* published in this Review for April, 1871, I have quoted passages from that letter; and here, considering the gravity of the assertions made by the *Quarterly* reviewer, I hope to be excused for re-quoting them :—

"Morality, properly so called—the science of right conduct—has for its object to determine *how* and *why* certain modes of conduct are detrimental, and certain other modes beneficial. These good and bad results cannot be accidental, but must be necessary consequences of the constitution of things; and I conceive it to be the business of Moral Science to deduce from the laws of life and the conditions of existence, what kinds of action necessarily tend to produce happiness, and what kinds to produce unhappiness. Having done this, its deductions are to be recognized as laws of conduct; and are to be conformed to irrespective of a direct estimation of happiness or misery."

* * * * * *

"If it is true that pure rectitude prescribes a system of things far too good for men as they are, it is not less true that mere expediency does not of itself tend to establish a system of things any better than that which exists. While absolute morality owes to expediency the checks which prevent it from rushing into Utopian absurdities, expediency is indebted to absolute morality for all stimulus to improvement. Granted that we are chiefly interested in ascertaining what is *relatively right,* it still follows that we must first consider what is *absolutely right;* since the one conception presupposes the other."

And the comment I then made on these passages I may make now, that "I do not see how there could well be a more emphatic assertion that there exists a primary basis of morals independent of, and in a sense antecedent to, that which is furnished by experiences of utility; and consequently independent of, and in a sense antecedent to, those moral sentiments which I conceive to be generated by such experiences." I will only add that, had my beliefs been directly opposite to those I have enun-

ciated, the reviewer might, I think, have found good reasons for his assertion. If, instead of demurring to the doctrine "that greatest happiness should be the *immediate* aim of man,"* I had endorsed that doctrine—if, instead of explaining and justifying "a belief in the special sacredness of these highest principles, and a sense of the supreme authority of the altruistic sentiments answering to them,"† I had denied the sacredness and the supreme authority—if, instead of saying of the wise man that "the highest truth he sees he will fearlessly utter; knowing that, let what may come of it, he is thus playing his right part in the world,"‡ I had said that the wise man will *not* do this; the reviewer might with truth have described me as not understanding "the term 'morality' according to the true sense of the word." And he might then have inferred that the Doctrine of Evolution as I hold it, implies denial of the "distinction between Duty and Pleasure." But as it is, I think the evidence will not generally be held to warrant his assertion.

I quite agree with the reviewer that the prevalence of a philosophy "is no mere question of speculative interest, but is one of the highest practical importance." I join him, too, in the belief that "calamitous social and political changes" may be the outcome of a mistaken philosophy. Moreover, writing as he does under the conviction that there can be no standard of right and wrong save one derived from a Revelation interpreted by an Infallible Authority, I can conceive the alarm with which he regards so radically-opposed a system. Though I could have wished that the sense of justice he generally displays had prevented him from ignoring the evidence I have above given, I can understand how, from his

* *Social Statics*, chap. iii. † *Principles of Psychology*, § 531.
‡ *First Principles*, § 34.

point of view, the Doctrine of Evolution, as I understand it, "seems absolutely fatal to every germ of morality," and "entirely negatives every form of religion." But I am unable to understand that modified Doctrine of Evolution which the reviewer hints at as an alternative. For, little as the reader would anticipate it after these expressions of profound dissent, the reviewer displays such an amount of agreement as to suggest that the system he is criticizing might be converted, "rapidly and without violence, into an 'allotropic state,' in which its conspicuous characters would be startlingly diverse from those that it exhibits at present." May I, using a different figure, suggest a different transformation, having a subjective instead of an objective character? As in a stereoscope, the two views representing diverse aspects, often yield at first a jumble of conflicting impressions, but after a time suddenly combine into a single whole which stands out quite clearly; so, may it not be that the seemingly-inconsistent Idealism and Realism dwelt on by the reviewer, as well as the other seemingly-fundamental incongruities he is struck by, will, under more persistent contemplation, unite as complementary sides of the same thing?

My excuse for devoting some space to a criticism of so entirely different a kind as that contained in the *British Quarterly Review* for October, must be that, under the circumstances, I cannot let it pass unnoticed without seeming to admit its validity.

Saying that my books should be dealt with by specialists, and tacitly announcing himself as an expert in Physics, the reviewer takes me to task both for errors in the statement of physical principles and for erroneous reasoning in physics. That he discovers no mistakes I

do not say. It would be marvellous if in such a multitude of propositions, averaging a dozen per page, I had made all criticism-proof. Some are inadvertencies which I should have been obliged to the reviewer for pointing out as such, but which he prefers to instance as proving my ignorance. In other cases, taking advantage of an imperfection of statement, he proceeds to instruct me about matters which either the context, or passages in the same volume, show to be quite familiar to me. Here is a sample of his criticisms belonging to this class :—

"Nor should we counsel a man to venture upon physical speculations who converts the proposition '*heat is insensible motion*' into '*insensible motion is heat*,' and hence concludes that when a force is applied to a mass so large that no motion is seen to result from it, or when, as in the case of sound, motion gets so dispersed that it becomes insensible, it turns to heat."

Respecting the first of the two statements contained in this sentence, I will observe that the reader, if not misled by the quotation-marks into the supposition that I have made, in so many words, the assertion that "insensible motion is heat," will at any rate infer that this assertion is distinctly involved in the passage named. And he will infer that the reviewer would never have charged me with such an absurd belief, if there was before him evidence proving that I have no such belief. What will the reader say, then, when he learns, not simply that there is no such statement, and not simply that on the page referred to, which I have ascertained to be the one intended, there is no such implication visible, even to an expert (and I have put the question to one); but when he further learns that in other passages, the fact that heat is one only of the modes of insensible motion is distinctly stated (see *First Prin.* §§ 66, 68, 171); and when he learns that elsewhere I have specified the several forms of insensible motion? If the re-

viewer, who looks so diligently for flaws as to search an essay in a volume he is not reviewing to find one term of an incongruity, had sought with equal diligence to learn what I thought about insensible motion, he would have found in the *Classification of the Sciences*, Table II., that insensible motion is described by me as having the forms of Heat, Light, Electricity, Magnetism. Even had there been in the place he names, an unquestionable implication of the belief which he ascribes to me, fairness might have led him to regard it as an oversight when he found it at variance with statements I have elsewhere made. What then is to be thought of him when, in the place named, no such belief is manifest; either to an ordinary reader or to a specially-instructed reader?

No less significant is the state of mind betrayed in the second clause of the reviewer's sentence. By representing me as saying that when the motion constituting sound "gets so dispersed that it becomes insensible, it turns to heat," does he intend to represent me as thinking that when sound-undulations become too weak to be audible, they become heat-undulations? If so, I reply that the passage he refers to has no such meaning. Does he then allege that some part of the force diffused in sound-waves is expended in generating electricity, by the friction of heterogeneous substances (which, however, eventually lapses from this special form of molecular motion in that general form constituting heat); and that I ought to have thus qualified my statement? If so, he would have had me commit a piece of scientific pedantry hindering the argument. If he does not mean either of these things, what does he mean? Does he contest the truth of the hypothesis which enabled Laplace to correct Newton's estimate of the velocity of sound—the hypothesis that heat is evolved by the com-

pression each sound-wave produces in the air? Does he deny that the heat so generated is at the expense of so much wave-motion lost? Does he question the inference that some of the motion embodied in each wave is from instant to instant dissipated, partly in this way and partly in the heat evolved by fluid friction? Can he show any reason for doubting that when the sound-waves have become too feeble to affect our senses, their motion still continues to undergo this transformation and diminution until it is all lost? If not, why does he implicitly deny that the molar motion constituting sound, eventually disappears in producing the molecular motion constituting heat? *

I will dwell no longer on the exclusively-personal questions raised by the reviewer's statements; but, leaving the reader to judge of the rest of my "stupendous mistakes" by the one I have dealt with, I will turn to a

* Only after the foregoing paragraphs were written, did the remark of a distinguished friend show me how certain words were misconstrued by the reviewer in a way that had never occurred to me as possible. In the passage referred to, I have said that sound-waves "finally die away in generating thermal undulations that radiate into space;" meaning, of course, that the force embodied in the sound-waves is finally *exhausted* in generating thermal undulations. In common speech, the dying-away of a prolonged sound, as that of a church-bell, includes its gradual diminution as well as its final cessation. But rather than suppose I gave to the words this ordinary meaning, the reviewer supposes me to believe, not simply that the *longitudinal* waves of air can pass, *without discontinuity*, into the *transverse* waves of ether, but he also debits me with the belief that the one order of waves, having lengths measurable in feet, and rates expressed in hundreds per second, can, *by mere enfeeblement*, pass into the other order of waves, having lengths of some fifty thousand to the inch, and rates expressed in many billions per second! Why he preferred so to interpret my words, and that, too, in the face of contrary implications elsewhere (instance § 100), will, however, be manifest to every one who reads his criticisms.

question worthy to occupy some space, as having an impersonal interest—the question, namely, respecting the nature of the warrant we have for asserting ultimate physical truths. The contempt which, as a physicist, the reviewer expresses for the metaphysical exploration of physical ideas, I will pass over with the remark that every physical question, probed to the bottom, opens into a metaphysical one; and that I should have thought the controversy now going on among chemists, respecting the legitimacy of the atomic hypothesis, might have shown him as much. On his erroneous statement that I use the phrase "Persistence of Force" as an equivalent for the now-generally-accepted phrase "Conservation of Energy," I will observe only that, had he not been in so great a hurry to find inconsistencies, he would have seen why, for the purposes of my argument, I intentionally use the word Force: Force being the generic word, including both that species known as Energy, and that species by which Matter occupies space and maintains its integrity—a species which, whatever may be its relation to Energy, and however clearly recognized as a necessary *datum* by the theory of Energy, is not otherwise considered in that theory. I will confine myself to the proposition, disputed at great length by the reviewer, that our cognition of the Persistence of Force is *à priori*. He relies much on the authority of Professor Tait, whom he twice quotes to the effect that—

> "Natural philosophy is an experimental, and not an intuitive science. No *à priori* reasoning can conduct us demonstratively to a single physical truth."

Were I to take a hypercritical attitude, I might dwell on the fact that Professor Tait leaves the extent of his proposition somewhat doubtful, by speaking of "Natural philosophy" as *one* science. Were I to follow further

the reviewer's example, I might point out that "Natural philosophy," in that Newtonian acceptation adopted by Professor Tait, includes Astronomy; and, going on to ask what astronomical "experiments" those are which conduct us to astronomical truths, I might then "counsel" the reviewer not to depend on the authority of one who (to use the reviewer's polite language) "blunders" by confounding experiment and observation. I will not, however, thus infer from Professor Tait's imperfection of statement that he is unaware of the difference between the two; and shall rate his authority as of no less value than I should, had he been more accurate in his expression. Respecting that authority I shall simply remark that, if the question had to be settled by the authority of any physicist, the authority of Mayer, who is diametrically opposed to Prof. Tait on this point, and who has been specially honoured, both by the Royal Society and by the French Institute, might well counterweigh his, if not out-weigh it. I am not aware, however, that the question is one in Physics. It seems to me a question respecting the nature of proof. And, without doubting Professor Tait's competence in Logic and Psychology, I should decline to abide by his judgment on such a question, even were there no opposite judgment given by a physicist, certainly of not less eminence.

Authority aside, however, let us discuss the matter on its merits. In the *Treatise on Natural Philosophy*, by Professors Thomson and Tait, § 243, I read that "as we shall show in our chapter on 'Experience,' physical axioms are axiomatic to those only who have sufficient knowledge of the action of physical causes to enable them to see at once their necessary truth." In this I agree entirely. It is in Physics, as it is in Mathematics, that before necessary truths can be grasped, there must

be gained by individual experience, such familiarity with the elements of the thoughts to be framed, that propositions about those elements may be mentally represented with distinctness. Tell a child that things which are equal to the same thing are equal to one another, and the child, lacking a sufficiently-abstract notion of equality, and lacking, too, the needful practice in comparing relations, will fail to grasp the axiom. Similarly, a rustic, never having thought much about forces and their results, cannot form a definite conception answering to the axiom that action and reaction are equal and opposite. In the last case as in the first, ideas of the terms and their relations require to be made, by practice in thinking, so vivid that the involved truths may be mentally seen. But when the individual experiences have been multiplied enough to produce distinctness in the representations of the elements dealt with; then, in the one case as in the other, those mental forms generated by ancestral experiences, cannot be occupied by the elements of one of these ultimate truths without perception of its necessity. If Professor Tait does not admit this, what does he mean by speaking of "physical *axioms*," and by saying that the cultured are enabled "to see *at once* their *necessary* truth?"

Again, if there are no physical truths which must be classed as *à priori*, I ask why Professor Tait joins Sir W. Thomson in accepting as bases for Physics, Newton's Laws of Motion? Though Newton gives illustrations of prolonged motion in bodies that are little resisted, he gives no *proof* that a body in motion will continue moving, if uninterfered with, in the same direction at the same velocity; nor, on turning to the enunciation of this law quoted in the above-named work, do I find that Professor Tait does more than exemplify it by facts

which can themselves be asserted only by taking the law for granted. Does Professor Tait deny that the first law of motion is a physical truth? If so, what does he call it? Does he admit it to be a physical truth, and, denying that it is *à priori*, assert that it is established *à posteriori*—that is, by conscious induction from observation and experiment? If so, what is the inductive reasoning which can establish it? Let us glance at the several conceivable arguments which we must suppose him to rely on.

A body set in motion soon ceases to move if it encounters much friction, or much resistance from the bodies struck. If less of its energy is expended in moving, or otherwise affecting, other bodies, or in overcoming friction, its motion continues longer. And it continues longest when, as over smooth ice, it meets with the smallest amount of obstruction. May we then, proceeding by the method of concomitant variations, infer that were it wholly unobstructed its motion would continue undiminished? If so, we assume that the diminution of its motion observed in experience, is proportionate to the amount of energy abstracted from it in producing other motion, either molar or molecular. We assume that no variation has taken place in its rate, save that caused by deductions in moving other matter; for if its motion be supposed to have otherwise varied, the conclusion that the differences in the distances travelled result from differences in the obstructions met with, is vitiated. Thus the truth to be established is already taken for granted in the premises. Nor is the question begged in this way only, In every case where it is remarked that a body stops the sooner, the more it is obstructed by other bodies or media, the law of inertia is assumed to

hold in the obstructing bodies or media. The very conception of greater or less retardation so caused, implies the belief that there can be no retardations without proportionate retarding causes; which is itself the assumption otherwise expressed in the first law of motion.

Again, let us suppose that instead of inexact observations made on the movements occurring in daily experience, we make exact experiments on movements specially arranged to yield measured results; what is the postulate underlying every experiment? Uniform velocity is defined as motion through equal spaces in equal times. How do we measure equal times? By an instrument which can be inferred to mark equal times only if the oscillations of the pendulum are isochronous; which they can be proved to be only if the first and second laws of motion are granted. That is to say, the proposed experimental proof of the first law, assumes not only the truth of the first law, but of that which Professor Tait agrees with Newton in regarding as a second law. Is it said that the ultimate time-measure referred to is the motion of the Earth round its axis, through equal angles in equal times? Then the obvious rejoinder is that the assertion of this, similarly involves an assertion of the truth to be proved; since the undiminished rotatory movement of the Earth is itself a corollary from the first law of motion. Is it alleged that this axial movement of the Earth through equal angles in equal times, is ascertainable by reference to the stars? I answer that a developed system of Astronomy, leading through complex reasonings to the conclusion that the Earth rotates, is, in that case, supposed to be needful before there can be established a law of motion which this system of Astronomy itself postulates. For even should it be said that the Newtonian theory of the Solar System is not neces-

sarily pre-supposed, but only the Copernican; still, the proof of this assumes that a body at rest (a star being taken as such) will continue at rest; which is a part of the first law of motion, regarded by Newton as not more self-evident than the remaining part.

Not a little remarkable, indeed, is the oversight made by Professor Tait, in asserting that "no *à priori* reasoning can conduct us demonstratively to a single physical truth," when he has before him the fact that the system of physical truths constituting Newton's *Principia*, which he has joined Sir William Thomson in editing, is established by *à priori* reasoning. That there can be no change without a cause, or, in the words of Mayer, that "a force cannot become nothing, and just as little can a force be produced from nothing," is that ultimate dictum of consciousness on which all physical science rests. It is involved alike in the assertion that a body at rest will continue at rest, in the assertion that a body in motion must continue to move at the same velocity in the same line if no force acts on it, and in the assertion that any divergent motion given to it must be proportionate to the deflecting force; and it is also involved in the axiom that action and reaction are equal and opposite.

The reviewer's doctrine, in support of which he cites against me the authority of Professor Tait, illustrates in Physics that same error of the inductive philosophy which, in Metaphysics, I have pointed out elsewhere (*Principles of Psychology*, Part VII.). It is a doctrine implying that we can go on for ever asking the proof of the proof, without finally coming to any deepest cognition which is unproved and unprovable. That this is an untenable doctrine, I need not say more to show. Nor, indeed, would saying more to show it be likely to have any effect, in so far at least as the reviewer is concerned;

seeing that he thinks I am "ignorant of the very nature of the principles" of which I am speaking, and seeing that my notions of scientific reasoning "remind" him "of the Ptolemists," who argued that the heavenly bodies must move in circles because the circle is the most perfect figure.*

Not to try the reader's patience further, I will end by pointing out that, even were the reviewer's criticisms all valid, they would leave unshaken the theory he contends against. Though one of his sentences (p. 480) raises the expectation that he is about to assault, and greatly to damage, the bases of the system contained in the second part of *First Principles*, yet all those propositions which constitute the bases, he leaves, not only uninjured, but even untouched,—contenting himself with trying to show (with what success we have seen) that the fundamental one is an *à posteriori* truth and not an *à priori* truth. Against the general Doctrine of Evolution, considered as an induction from all classes of concrete

* Other examples of these amenities of controversy, in which I decline to imitate my reviewer, have already been given. What occasions he supplies me for imitation, were I minded to take advantage of them, an instance will show. Pointing out an implication of certain reasonings of mine, he suggests that it is too absurd even for me to avow explicitly; saying:—"We scarcely think that even Mr. Spencer will venture to claim as a datum of consciousness the Second Law of Motion, with its attendant complexities of component velocities, &c." Now any one who turns to Newton's *Principia*, will find that to the enunciation of the Second Law of Motion, nothing whatever is appended but an amplified re-statement—there is not even an illustration, much less a proof. And from this law, this axiom, this immediate intuition or "datum of consciousness," Newton proceeds forthwith to draw those corollaries respecting the composition of forces which underlie all dynamics. What, then, must be thought of Newton, who explicitly assumes that which the reviewer thinks it absurd to assume implicitly?

phenomena, he utters not a word; nor does he utter a word to disprove any one of those laws of the redistribution of matter and motion, by which the process of Evolution is deductively interpreted. Respecting the law of the Instability of the Homogeneous, he says no more than to quarrel with one of the illustrations. He makes no criticism on the law of the Multiplication of Effects. The law of Segregation he does not even mention. Nor does he mention the law of Equilibration. Further, he urges nothing against the statement that these general laws are severally deducible from the ultimate law of the Persistence of Force. Lastly, he does not deny the Persistence of Force; but only differs respecting the nature of our warrant for asserting it. Beyond pointing out, here a cracked brick and there a coin set askew, he merely makes a futile attempt to show that the foundation is not natural rock, but concrete.

From his objections I may, indeed, derive much satisfaction. That a competent critic, obviously anxious to do all the mischief he can, and not over-scrupulous about the means he uses, has done so little, may be taken as evidence that the fabric of conclusions attacked will not be readily overthrown.

In the *British Quarterly Review* for January, 1874, the writer of the article I have dealt with above, makes a rejoinder. It is of the kind which might have been anticipated. There are men to whom the discovery that they have done injustice is painful. After proof of having wrongly ascribed to another such a nonsensical belief as that insensible motion is heat because heat is insensible motion, some would express regret. Not so my reviewer. Having by forced interpretations debited

me with an absurdity, he makes no apology; but, with an air implying that he had all along done this, he attacks the allegation I had really made—an allegation which is at least so far from an absurdity, that he describes it only as not justified by "the present state of science." And here, having incidentally referred to this point, I may as well, before proceeding, deal with his substituted charge at the same time that I further exemplify his method. Probably most of those who see the *British Quarterly*, will be favourably impressed by the confidence of his assertion; but those who compare my statement with his travesty of it, and who compare both with some authoritative exposition, will be otherwise impressed. To his statement that I conclude "that friction must ultimately transform *all* [the italics are his] the energy of a sound into heat," I reply that it is glaringly untrue: I have named friction as a second cause. And when he pooh-poohs the effect of compression because it is "merely momentary," is he aware of the meaning of his words? Will he deny that, from first to last, during the interval of condensation, heat is being generated? Will he deny to the air the power of radiating such heat? He will not venture to do so. Take then the interval of condensation as one-thousandth of a second. I ask him to inform those whom he professes to instruct, what is the probable number of heat-waves which have escaped in this interval. Must they not be numbered by thousands of millions? In fact, by his "merely momentary," he actually assumes that what is momentary in relation to our time-measures, is momentary in relation to the escape of ethereal undulations!

Let me now proceed more systematically, and examine his rejoinder point by point. It sets out thus:—

"In the notice of Mr. Spencer's works that appeared in the last

number of this *Review*, we had occasion to point out that he held mistaken notions of the most fundamental generalizations of dynamics; that he had shown an ignorance of the nature of proof in his treatment of the Newtonian Law; that he had used phrases such as the Persistence of Force in various and inconsistent significations; and more especially that he had put forth proofs logically faulty in his endeavour to demonstrate certain physical propositions by *à priori* methods, and to show that such proofs must exist. To this article Mr. Spencer has replied in the December number of the *Fortnightly Review*. His reply leaves every one of the above positions unassailed."

In my "Replies to Criticisms," which, as it was, trespassed unduly on the pages of the *Fortnightly Review,* I singled out from his allegations which touched me personally, one that might be briefly dealt with as an example; and I stated that, passing over other personal questions, as not interesting to the general reader, I should devote the small space available to an impersonal one. Notwithstanding this, the reviewer, in the foregoing paragraph, enumerates his chief positions; asserts that I have not assailed any of them (which is untrue); and then leads his readers to the belief that I have not assailed them because they are unassailable.

Leaving this misbelief to be dealt with presently, I continue my comments on his rejoinder. After referring to the passage I have quoted from Prof. Tait's statement about physical axioms, and after indicating the nature of my criticism, the reviewer says:—

"Had Mr. Spencer, however, read the sentence that follows it, we doubt whether we should have heard aught of this quotation. It is 'Without further remark we shall give Newton's Three Laws; it being remembered that as the properties of matter might have been such as to render a totally different set of laws axiomatic, *these laws must be considered as resting on convictions drawn from observation and experiment and not on intuitive perception*.' This not only shows that the term 'axiomatic' is used in the previous sentence in a sense that does not exclude an inductive origin, but it leaves us indebted

to Mr. Spencer for the discovery of the clearest and most authoritative expression of disapproval of his views respecting the nature of the Laws of Motion."

Let us analyze this "authoritative expression." It contains several startling implications, the disclosure of which the reader will find not uninteresting. Consider, first, what is implied by framing the thought that "the properties of matter might have been such as to render a totally different set of laws axiomatic." I will not stop to make the inquiry whether matter having properties fundamentally unlike its present ones, can be conceived; though such an inquiry, leading to the conclusion that no conception of the kind is possible, would show that the proposition is merely a verbal one. It will suffice if I examine the nature of this proposition that "the properties of matter *might have been*" other than they are. Does it express an experimentally-ascertained truth? If so, I invite Prof. Tait to describe the experiments. Is it an intuition? If so, then along with doubt of an intuitive belief concerning things *as they are*, there goes confidence in an intuitive belief concerning things *as they are not*. Is it an hypothesis? If so, the implication is that a cognition of which the negation is inconceivable (for an axiom is such) may be discredited by inference from that which is not a cognition at all, but simply a supposition. Does the reviewer admit that no conclusion can have a validity greater than is possessed by its premises? or will he say that the trustworthiness of cognitions increases in proportion as they are the more inferential? Be his answer what it may, I shall take it as unquestionable that nothing concluded can have a warrant higher than that from which it is concluded, though it may have a lower. Now the elements of the proposition before us are these:—*As* "the properties of matter might have

been such as to render a totally different set of laws axiomatic" [*therefore*] "these laws [now in force] must be considered as resting . . . not on intuitive perception:" that is, the intuitions in which these laws are recognized, must not be held authoritative. Here the cognition posited as premiss, is that the properties of matter might have been other than they are; and the conclusion is that our intuitions relative to existing properties are uncertain. Hence, if this conclusion is valid, it is valid because the cognition or intuition respecting what might have been, is more trustworthy than the cognition or intuition respecting what is! Scepticism respecting the deliverances of consciousness about things as they are, is based upon faith in a deliverance of consciousness about things as they are not!

I go on to remark that this "authoritative expression of disapproval" by which I am supposed to be silenced, even were its allegation as valid as it is fallacious, would leave wholly untouched the real issue. I pointed out how Prof. Tait's denial that any physical truths could be reached *à priori*, was contradicted by his own statement respecting physical axioms. The question thus raised the reviewer evades, and substitutes another with which I have just dealt. Now I bring forward again the evaded question.

In the passage I quoted, Prof. Tait, besides speaking of physical "*axioms*," says of them that due familiarity with physical phenomena gives the power of seeing "*at once*" "their *necessary* truth." These last words, which express his conception of an axiom, express also the usual conception. An axiom is defined as a "self-evident truth," or a truth that is seen *at once;* and the definition otherwise worded is—a "truth so evident *at first sight*, that no process of reasoning or demonstra-

tion can make it plainer." Now I contend that Prof. Tait, by thus committing himself to a definition of physical axioms identical with that which is given of mathematical axioms, tacitly admits that they have the same *à priori* character; and I further contend that no such nature as that which he describes physical axioms to have, can be acquired by experiment or observation during the life of an individual. Axioms, if defined as truths of which the *necessity* is at once seen, are thereby defined as truths of which the negation is inconceivable; and the familiar contrast between them and the truths established by individual experiences, is that these last never become such that their negations are inconceivable, however multitudinous the experiences may be. Thousands of times has the sportsman heard the report that follows the flash from his gun, but still he can imagine the flash as occurring silently; and countless daily experiments on the burning of coal, leave him able to conceive coal as remaining in the fire without ignition. So that the "convictions drawn from observation and experiment" during a single life, can never acquire that character which Prof. Tait admits physical axioms to have: in other words, physical axioms cannot be derived from personal observation and experiment. Thus, otherwise applying the reviewer's words, I "doubt whether we should have heard aught of this quotation" to which he calls my attention, had he studied the matter more closely: and he "leaves us indebted to" him "for the discovery of" a passage which serves to make clearer the untenability of the doctrine he so dogmatically affirms.

I turn now to what the reviewer says concerning the special arguments I used to show that the first law of

motion cannot be proved experimentally. After a bare enunciation of my positions, he says :—

"On the utterly erroneous character of these statements we do not care to dwell, we wish simply to call our reader's attention to the conclusion arrived at. Is that a disproof of the possibility of an inductive proof? We thought that every tolerably educated man was aware that the proof of a scientific law *consisted in* showing that *by* assuming its truth, we could explain the observed phenomena."

Probably the reviewer expects his readers to conclude that he could easily dispose of the statements referred to if he tried. Among scientific men, however, this cavalier passing over of my arguments will perhaps be ascribed to another cause. I will give him my reason for saying this. Those arguments, read in proof by one of the most eminent physicists, and by a specially-honoured mathematician, had their entire concurrence; and I have since had from another mathematician, standing among the very first, such qualified agreement as is implied in saying that the first law of motion cannot be proved by terrestrial observations (which is in large measure what I undertook to show in the paragraphs which the reviewer passes over so contemptuously). But his last sentence, telling us what he thought "every tolerably educated man was aware" of, is the one which chiefly demands attention. In it he uses the word *law*—a word which, conveniently wide in meaning, suits his purpose remarkably well. But we are here speaking of physical *axioms*. The question is whether the justification of a physical axiom consists in showing that by assuming its truth, we can explain the observed phenomena. If it does, then all distinction between hypothesis and axiom disappears. Mathematical axioms, for which there is no other definition than that which Prof. Tait gives of physical axioms, must stand on the same footing. Henceforth we must

hold that our warrant for asserting that "things which are equal to the same thing are equal to one another," consists in the observed truth of the geometrical and other propositions deducible from it and the associated axioms—the *observed* truth, mind; for the fabric of deductions yields none of the required warrant until these deductions have been tested by measurement. When we have described squares on the three sides of a right-angled triangle, cut them out in paper, and, by weighing them, have found that the one on the hypothenuse balances the other two; then we have got a fact which, joined with other facts similarly ascertained, justifies us in asserting that things which are equal to the same thing are equal to one another! Even as it stands, this implication will not, I think, be readily accepted; but we shall find that its unacceptability becomes still more conspicuous when the analysis is pursued to the end.

Continuing his argument to show that the laws of motion have no *à priori* warrant, the reviewer says:—

"Mr. Spencer asserts that Newton gave no proof of the Laws of Motion. The whole of the *Principia* was the proof, and the fact that, taken as a system, these laws account for the lunar and planetary motions, is the warrant on which they chiefly rest to this day."

I have first to point out that here, as before, the reviewer escapes by raising a new issue. I did not ask what he thinks about the *Principia*, and the proof of the laws of motion by it; nor did I ask whether others at this day, hold the assertion of these laws to be justified mainly by the evidence the Solar System affords. I asked what Newton thought. The reviewer had represented the belief that the second law of motion is knowable *à priori*, as too absurd even for me openly to enunciate. I pointed out that since Newton enunciates it openly under the title of an axiom, and offers no proof whatever of it, he did explicitly what I am blamed for

doing implicitly. And thereupon I invited the reviewer to say what he thought of Newton. Instead of answering, he gives me his opinion to the effect that the laws of motion are proved true by the truth of the *Principia* deduced from them. Of this hereafter. My present purpose is to show that Newton did not say this, and gave every indication of thinking the contrary. He does not call the laws of motion "hypotheses;" he calls them "axioms." He does not say that he assumes them to be true *provisionally;* and that the warrant for accepting them as actually true, will be found in the astronomically-proved truth of the deductions. He lays them down just as mathematical axioms are laid down—posits them as truths to be accepted *à priori*, from which follow consequences which must therefore be accepted. And though the reviewer thinks this an untenable position, I am quite content to range myself with Newton in thinking it a tenable one—if, indeed, I may say so without undervaluing the reviewer's judgment. But now, having shown that the reviewer evaded the issue I raised, which it was inconvenient for him to meet, I pass to the issue he substitutes for it. I will first deal with it after the methods of ordinary logic, before dealing with it after the methods of what may be called transcendental logic.

To establish the truth of a proposition postulated, by showing that the deductions from it are true, requires that the truth of the deductions shall be shown in some way that does not directly or indirectly assume the truth of the proposition postulated. If, setting out with the axioms of Euclid, we deduce the truths that "the angle in a semicircle is a right angle," and that "the opposite angles of any quadrilateral figure described in a circle, are together equal to two right angles," and so forth; and if, because these propositions are true, we say that the

axioms are true, we are guilty of a *petitio principii*. I do not mean simply that if these various propositions are taken as true on the strength of the demonstrations given, the reasoning is circular, because the demonstrations assume the axioms; but I mean more—I mean that any supposed *experimental* proof of these propositions, by measurement, itself assumes the axioms to be justified. For even when the supposed experimental proof consists in showing that some two lines demonstrated by reason to be equal, are equal when tested in perception, the axiom that things which are equal to the same thing are equal to one another, is taken for granted. The equality of the two lines can be ascertained only by carrying from the one to the other, some measure (either a moveable marked line or the space between the points of compasses), and by assuming that the two lines are equal to one another, because they are severally equal to this measure. The ultimate truths of mathematics, then, cannot be established by any experimental proof that the deductions from them are true; since the supposed experimental proof takes them for granted. The same thing holds of ultimate physical truths. For the alleged *à posteriori* proof of these truths, has a vice exactly analogous to the vice I have just indicated. Every evidence yielded by astronomy that the axioms called "the laws of motion" are true, resolves itself into a fulfilled prevision that some celestial body or bodies, will be seen in a specified place, or in specified places, in the heavens, at some assigned time. Now the day, hour, and minute of this verifying observation, can be fixed only on the assumption that the Earth's motion in its orbit and its motion round its axis, continue undiminished. Mark, then, the parallelism. One who chose to deny that things which are equal to the same thing are equal to one another, could never have it

proved to him by showing the truth of deduced propositions; since the testing process would in every case assume that which he denied. Similarly, one who refused to admit that motion, uninterfered with, continues in the same straight line at the same velocity, could not have it proved to him by the fulfilment of an astronomical prediction; because he would say that both the spectator's position in space, and the position of the event in time, were those alleged, only if the Earth's motions of translation and rotation were undiminished, which was the very thing he called in question. Evidently such a sceptic might object that the seeming fulfilment of the prediction, say a transit of Venus, may be effected by various combinations of the changing positions of Venus, of the Earth, and of the spectator on the Earth. The appearances may occur as anticipated, though Venus is at some other place than the calculated one; provided the Earth also is at some other place, and the spectator's position on the Earth is different. And if the first law of motion is not assumed, it must be admitted that the Earth and the spectator *may* occupy these other places at the predicted time: supposing that in the absence of the first law, this predicted time can be ascertained, which it cannot. Thus the testing process inevitably begs the question.

That the perfect congruity of all astronomical observations with all deductions from "the laws of motion," gives coherence to this group of intuitions and perceptions, and so furnishes a warrant for the entire aggregate of them which it would not have were any of them at variance, is unquestionable. But it does not therefore follow that astronomical observations can furnish a test for *each individual assumption*, out of the many which are simultaneously made. I will not dwell on the fact that the process of verification assumes the validity

of the assumptions on which acts of reasoning proceed; for the reply may be that these are shown to be valid apart from astronomy. Nor will I insist that the assumptions underlying mathematical inferences, geometrical and numerical, are involved; since it may be said that these are justifiable separately by our terrestrial experiences. But, passing over all else that is taken for granted, it suffices to point out that, in making every astronomical prediction, the three laws of motion and the law of gravitation are *all* assumed; that if the first law of motion is to be held proved by the fulfilment of the prediction, it can be so only by taking for granted that the two other laws of motion and the law of gravitation are true; and that non-fulfilment of the prediction would not disprove the first law of motion, since the error might be in one or other of the three remaining assumptions. Similarly with the second law: the astronomical proof of it depends on the truth of the accompanying assumptions. So that the warrants for the assumptions A, B, C, and D, are respectively such that A, B, and C, being taken as trustworthy, prove the validity of D; D being thus proved valid, joins C, and B, in giving a character to A; and so throughout. The result is that everything comes out right if they happen to be all true; but if one of them is false, it may destroy the characters of the other three, though these are in reality exact. Clearly, then, astronomical prediction and observation can never test any one of the premises by itself. They can only justify the entire aggregate of premises, mathematical and physical, joined with the entire aggregate of reasoning processes leading from premises to conclusions.

I now recall the reviewer's "thought," uttered in his habitual manner, "that every tolerably educated man was

aware that the proof of a scientific law *consisted in* showing that *by* assuming its truth, we could explain the observed phenomena." Having from the point of view of ordinary logic dealt with this theory of proof as applied by the reviewer, I proceed to deal with it from the point of view of transcendental logic, as I have myself applied it. And here I have to charge the reviewer with either being ignorant of, or else deliberately ignoring, a cardinal doctrine of the System of Philosophy he professes to review—a doctrine set forth not in those four volumes of it which he seems never to have looked into; but in the one volume of it he has partially dealt with. For this principle which, in respect to scientific beliefs, he enunciates for my instruction, is one which, in *First Principles*, I have enunciated in respect to all beliefs whatever. In the chapter on the "Data of Philosophy," where I have inquired into the legitimacy of our modes of procedure, and where I have pointed out that there are certain ultimate conceptions without which the intellect can no more stir "than the body can stir without help of its limbs," I have inquired how their validity or invalidity is to be shown; and I have gone on to reply that—

> "Those of them which are vital, or cannot be severed rom the rest without mental dissolution, must be assumed as true *provisionally* . . . leaving the assumption of their unquestionableness to be justified by the results.
>
> "§ 40. How is it to be justified by the results? As any other assumption is justified—by ascertaining that all the conclusions deducible from it, correspond with the facts as directly observed—by showing the agreement between the experiences it leads us to anticipate, and the actual experiences. There is no mode of establishing the validity of any belief, except that of showing its entire congruity with all other beliefs."

Proceeding avowedly and rigorously on this principle,

I have next inquired what is the fundamental *process* of thought by which this congruity is to be determined, and what is the fundamental *product* of thought yielded by this process. This fundamental product I have shown to be the coexistence of subject and object; and then, describing this as a postulate to be justified by "its subsequently-proved congruity with every result of experience, direct and indirect," I have gone on to say that "the two divisions of self and not-self, are re-divisible into certain most general forms, the reality of which Science, as well as Common Sense, from moment to moment assumes." Nor is this all. Having thus assumed, *only provisionally*, this deepest of all intuitions, far transcending an axiom in self-evidence, I have, after drawing deductions occupying four volumes, deliberately gone back to the assumption (*Prin. of Psy.*, § 386). After quoting the passage in which the principle was laid down, and after reminding the reader that the deductions drawn had been found congruous with one another; I have pointed out that it still remained to ascertain whether this primordial assumption was congruous with all the deductions; and have thereupon proceeded, throughout eighteen chapters, to show the congruity. And yet having before him the volumes in which this principle is set forth with a distinctness, and acted upon with a deliberation, which I believe are nowhere exceeded, the reviewer enunciates for my benefit this principle of which he "thought that every tolerably educated man was aware"! He enunciates it as applying to limited groups of beliefs, to which it does not apply; and shuts his eyes to the fact that I have avowedly and systematically acted upon it in respect to the entire aggregate of our beliefs (axioms included) for which it furnishes the ultimate justification!

Here I must add another elucidatory statement.

which would have been needless had the reviewer read that which he criticizes. His argument proceeds throughout on the assumption that I understand *à priori* truths after the ancient manner, as truths independent of experience; and he shows this more than tacitly, where he "trusts" that he is "attacking one of the last attempts to deduce the laws of nature from our inner consciousness." Manifestly, a leading thesis of one of the works he professes to review, is entirely unknown to him—the thesis that forms of thought, and consequently the intuitions which those forms of thought involve, result entirely from the effects of experiences, organized and inherited. With the *Principles of Psychology* before him, not only does he seem unaware that it contains this doctrine, but though this doctrine, set forth in its first edition published nearly twenty years ago, has gained considerable currency, he seems never to have heard of it. The implication of this doctrine is, not that the "laws of nature" are deducible from "our inner consciousness," but that our consciousness has a pre-established correspondence with such of those laws (simple, perpetually presented, and never negatived) as have, in the course of practically-infinite ancestral experiences, registered themselves in our nervous structure. Had he taken the trouble to acquaint himself with this doctrine, he would have learned that the intuitions of axiomatic truths are regarded by me as latent in the inherited brain, just as bodily reflex actions are latent in the inherited nervous centres of a lower order; that such latent intuitions are made potentially more distinct by the greater definiteness of structure due to individual action and culture; and that thus, axiomatic truths, having a warrant entirely *à posteriori* for the race, have for the individual a warrant which, substantially *à priori*, is made complete *à posteriori*. And

he would then have learnt that as, during evolution, Thought has been moulded into increasing correspondence with Things; and as such correspondence, tolerably complete in respect of the simple, ever-present, and invariable relations, as those of space, has made considerable advance in respect of the primary dynamical relations; the assertion that the resulting intuitions are authoritative, is the assertion that the simplest uniformities of nature, as experienced throughout an immeasurable past, are better known than they are as experienced during an individual life. All which conceptions, however, being, as it seems, unheard of by the reviewer, he regards my trust in these primordial intuitions as like that of the Ptolemists in their fancies about perfection!

Thus far my chief antagonists, passive if not active, have been Prof. Tait and, by implication, Sir William Thomson, his coadjutor in the work quoted against me—men of standing, and the last of them of world-wide reputation as a mathematician and physicist. Partly because the opinions of such men demand attention, I have dealt with the questions raised at some length; and partly, also, because the origin and consequent warrant of physical axioms are questions of general and permanent interest. The reviewer, who by citing against me these authorities has gained for some of his criticisms consideration they would otherwise not deserve, I must, in respect of his other criticisms, deal with very briefly. Because, for reasons sufficiently indicated, I did not assail sundry of his statements, he has reiterated them as unassailable. I will here add no more than is needful to show how groundless is his assumption.

What the reviewer says on the metaphysical aspects of the propositions we distinguish as physical, need not

detain us long. His account of my exposition of "Ultimate Scientific Ideas," he closes by saying of me that "he is not content with less than showing that all our fundamental conceptions are inconceivable." Whether the reviewer knows what he means by an inconceivable conception, I cannot tell. It will suffice to say that I have attempted no such remarkable feat as that described. My attempt has been to show that objective activities, together with their objective forms, are inconceivable by us—that such symbolic conceptions of them as we frame, and are obliged to use, are proved, by the alternative contradictions which a final analysis of them discloses, to have no likeness to the realities. But the proposition that objective existence cannot be rendered in terms of subjective existence, the reviewer thinks adequately expressed by saying that "our fundamental conceptions" (subjective products) "are inconceivable" (cannot be framed by subjective processes)! Giving this as a sample from which may be judged his fitness for discussing these ultimate questions, I pass over his physico-metaphysical criticisms, and proceed at once to those which his special discipline may be assumed to render more worthy of attention.

Quoting a passage relative to the law that "all central forces vary inversely as the squares of the distances," he derides the assertion that "this law is not simply an empirical one, but one deducible mathematically from the relations of space—one of which the negation is inconceivable." Now whether this statement can or cannot be fully justified, it has at any rate none of that absurdity alleged by the reviewer. When he puts the question—"Whence does he [do I] get this?" he invites the suspicion that his mind is not characterized by much excursiveness. It seems never to have occurred to him that,

if rays like those of light radiate in straight lines from a centre, the number of them falling on any given area of a sphere described from that centre, will diminish as the square of the distance increases, because the surfaces of spheres vary as the squares of their radii. For, if this has occurred to him, why does he ask whence I get the inference? The inference is so simple a one as naturally to be recognized by those whose thoughts go a little beyond their lessons in geometry.* If the reviewer means to ask, whence I get the implied assumption that central forces act only in straight lines, I reply that this assumption has a warrant akin to that of Newton's first axiom, that a moving body will continue moving in a straight line unless interfered with. For that the force exerted by one centre on another should act in a curved line, implies the conception of some second force, complicating the direct effect of the first. And, even could a central force be truly conceived as acting in lines not straight, the *average* distribution of its effects upon the inner surface of the surrounding sphere, would still follow the same law. Thus, whether or not the law be accepted on *à priori* grounds, the assumed absurdity of representing it to have *à priori* grounds, is not very obvious. Respecting this statement of mine the reviewer goes on to say—

"This is a wisdom far higher than that possessed by the discoverer of the great law of attraction, who was led to consider it from no cogitations on the relations of space, but from observations of the

* That I am certainly not singular in this view, is shown to me, even while I write, by the just-issued work of Prof. Jevons on the *Principles of Science: a Treatise on Logic and Scientific Method.* In vol. ii., p. 141, Prof. Jevons remarks respecting the law of variation of the attractive force, that it "is doubtless connected at this point with the primary properties of space itself, and is so far conformable to our necessary ideas."

movements of the planets; and who was so far from rising to that clearness of view of the truth of his great discovery, which is expressed by the phrase, 'its negation is inconceivable,' that he actually abandoned it for a time, because (through an error in his estimate of the earth's diameter) it did not seem fully to account for the motion of the moon."

To the first clause in this sentence, I have simply to give a direct denial; and to assert that neither Newton's "observations of the movements of the planets" nor other such observations continued by all astronomers for all time, would yield "the great law of attraction." Contrariwise, I contend that when the reviewer says, by implication, that Newton had no antecedent hypothesis respecting the cause of the planetary motions, he (the reviewer) is not only going beyond his possible knowledge, but he is asserting that which even a rudimentary acquaintance with the process of discovery, might have shown him was impossible. Without framing, beforehand, the supposition that there was at work an attractive force varying inversely as the square of the distance, no such comparison of observations as that which led to the establishment of the theory of gravitation could have been made. On the second clause of the sentence, in which the reviewer volunteers for my benefit the information that Newton "actually abandoned" his hypothesis for a while because it did not bring out right results, I have first to tell him that, in an early number of the very periodical containing his article,* I cited this fact (using these same words) at a time when he was at school, or before he went there.† I have next to assert

* See Essay on "The Genesis of Science," in the *British Quarterly Review* for July, 1854, p. 127.

† I do not say this at random. The reviewer, who has sought rather to make known than to conceal his identity, took his degree in 1868.

that this fact is irrelevant; and that Newton, while probably seeing it to be a necessary implication of geometrical laws that central forces vary inversely as the squares of the distances, did not see it to be a necessary implication of any laws, geometrical or dynamical, that there exists a force by which the celestial bodies affect one another; and therefore doubtless saw that there was no *à priori* warrant for the doctrine of gravitation. The reviewer, however, aiming to substitute for my "confused notions" his own clear ones, wishes me to identify the proposition—Central forces vary inversely as the squares of the distances—with the proposition—There is a cosmical force which varies inversely as the squares of the distances. But I decline to identify them; and I suspect that a considerable distinction between them was recognized by Newton. Lastly, apart from all this, I have to point out that even had Newton thought the existence of an attractive force throughout space was an *à priori* truth, as well as the law of variation of such a force if it existed; he would still, naturally enough, pause before asserting this law, when he found his deductions from it did not correspond with the facts. To suppose otherwise, is to ascribe to him a rashness which no disciplined man of science could be guilty of.

See, then, the critical capacity variously exhibited in the space of a single sentence. The reviewer, quite erroneously, thinks that observations unguided by hypotheses suffice for physical discoveries. He seems unaware that, on *à priori* grounds, the law of the inverse square had been suspected as the law of some cosmical force, before Newton. He asserts, without warrant, that no such *à priori* conception preceded, in Newton's mind, his observations and calculations. He confounds the law of variation of a force, with the existence of a force varying

according to that law. And he concludes that Newton could have had no *à priori* conception of the law of variation, because he did not assert the existence of a force varying according to this law in defiance of the evidence as then presented to him!

Now that I have analyzed, with these results, the first of his criticisms, the reader will neither expect me to waste time in similarly dealing with the rest *seriatim*, nor will he wish to have his own time occupied in following the analysis. To the evidence thus furnished of the reviewer's fitness for the task he undertakes, it will suffice if I add an illustration or two of the *animus* which leads him to make grave imputations on trivial grounds, and to ignore the evidence which contradicts his interpretations.

Because I have spoken of a balanced system, like that formed by the sun and planets, as having the "peculiarity, that though the constituents of the system have relative movements, the system, as a whole, has no movement," he unhesitatingly assumes me to be unaware that in a system of bodies whose movements are not balanced, it is equally true that the centre of gravity remains constant. Ignorance of a general principle in dynamics is alleged against me solely because of this colloquial use of the word "peculiarity," where I should have used a word (and there is no word perfectly fit) free from the implication of exclusiveness. If the reviewer were to assert that arrogance is a "peculiarity" of critics; and if I were thereupon to charge him with entire ignorance of mankind, many of whom besides critics are arrogant, he would rightly say that my conclusion was a very large one to draw from so small a premise.

To this example of strained inference I will join an example of what seems like deliberate misconstruc-

tion. From one of my essays (not among the works he professes to deal with) the reviewer, to strengthen his attack, brings a strange mistake; which, even without inquiry, any fair-minded reader would see must be an oversight. A statement true of a single body acted on by a tractive force, I have inadvertently pluralized: being so possessed by another aspect of the question, as to overlook the obvious fact that with a plurality of bodies the statement became untrue. Not only, however, does the reviewer ignore various evidences furnished by the works before him, that I could not really think what I had there said, but he ignores a direct contradiction contained in the paragraph succeeding that from which he quotes. So that the case stands thus:—On two adjacent pages I have made two opposite statements, both of which I cannot be supposed to believe. One of them is right; and this the reviewer assumes I do not believe. One of them is glaringly wrong; and this the reviewer assumes I do believe. Why he made this choice no one who reads his criticism will fail to see.

Even had his judgments more authority than is given to them by his mathematical honours, this brief characterization would, I think, suffice. Perhaps already, in rebutting the assumption that I did not answer his allegations because they were unanswerable, I have ascribed to them an unmerited importance. For the rest, suggesting that their value may be measured by the value of that above dealt with as a sample, I leave them to be answered by the works they are directed against.

Here I end. The foregoing pages, while serving, I think, the more important purpose of making clearer the relations of physical axioms to physical knowledge, incidentally justify the assertion that the reviewer's charges of fallacious reasoning and ignorance of the nature of

proof, recoil on himself. When, in his confident way, he undertakes to teach me the nature of our warrant for scientific beliefs, ignoring absolutely the inquiry contained in *Principles of Psychology,* concerning the relative values of direct intuitions and reasoned conclusions, he lays himself open to a sarcasm which is sufficiently obvious. And when a certain ultimate principle of justification for our beliefs, set forth and acted upon in the *System of Synthetic Philosophy* more distinctly than in any other work, is enunciated by him for my instruction, as one which he "thought that every tolerably educated man was aware" of, his course is one for which I find no fit epithet in the vocabulary I permit myself to use. That in some cases he has shown eagerness to found charges on misinterpretations little less than deliberate, has been sufficiently shown; as also that, in other cases, his own failure to discriminate is made the ground for ascribing to me beliefs that are manifestly untenable. Save in the single case of a statement respecting collisions of bodies, made by me without the needful qualification, I am not aware of any errors he detects, except errors of oversight or those arising from imperfect expression and inadequate exposition. When he unhesitatingly puts the worst constructions on these, it cannot be because his own exactness is such that no other constructions occur to him; for he displays an unusual capacity for inadvertencies, and must have had many experiences showing him how much he might be wronged by illiberal interpretations of them. One who in twenty-three professed extracts makes fifteen mistakes—words omitted, or added, or substituted—should not need reminding how largely mere oversight may raise suspicion of something worse. One who shows his notions of accurate statement by asserting that as I substitute "persistence"

for "conservation," I therefore identify Persistence of *Force* with Conservation of *Energy*, and debits me with the resulting incongruities—one who, in pursuance of this error, confounds a special principle with the general principle it is said to imply, and thereupon describes a wider principle as being included in a narrower (p. 481)—one who speaks of our "inner consciousness" (p. 488), so asserting, by implication, that we have an outer consciousness—one who talks of an inconceivable conception; ought surely to be aware how readily lax expressions may be turned into proofs of absurd opinions. And one who, in the space of a few pages, falls into so many solecisms, ought to be vividly conscious that a whole volume thus written would furnish multitudinous statements from which a critic, moved by a spirit like his own, might evolve abundant absurdities; supplying ample occasion for blazoning the tops of pages with insulting words.

[*A letter, drawn from Prof. Tait by the foregoing criticisms, and published by him in* Nature, *initiated a controversy carried on in that periodical for some months. Partly in justification of my position, and partly as tending to make clearer the nature and origin of physical axioms, I append certain portions of the correspondence, with some additional explanations and comments.*]

THESES.

1. *If A produces B, then* 2 *A will produce* 2 *B.*

This is the blank form of causal relation quantitatively considered, when the causes and effects are simple—that is, are unimpeded by other causes and uncomplicated by other effects; and whenever two or more causes co-operate, there is no possibility of determining the relation between the compound cause and the compound effect except by assuming that between each co-operating cause and its separate effect there exists this same quantitative relation.

2. *This truth holds whatever the natures of the simple causes and simple effects; and is an* à priori *assumption made in conducting every experiment and in reasoning from it.*

Every process of weighing, every chemical analysis, every physical investigation, proceeds on this truth without assigning warrant for it; and in allowing for the effect of any minor cause that interferes with the major cause, this same truth is assumed.

3. *When A is an impressed force and B the produced motion, then the general truth that if A produces B,* 2 *A will produce* 2 *B, becomes the more special truth called the Second Law of Motion.*

Newton's amplified statement of this Law is—"If any force generates a motion, a double force will generate double the motion, a triple force triple the motion, whether that force be impressed altogether and at once, or gradually and successively." And his further clause, asserting that this law holds whether the directions of the forces are or are not the same, asserts a proportionality between each force and its produced motion, such as we have seen to be invariably assumed between each cause and its separate effect, when there are co-operating causes.

4. *This Law may be affirmed, without specification of the modes in which the impressed force and the resulting motion are to be estimated.*

Newton's statement is abstract. Taking for granted right modes of measurement, it asserts that the alteration of motion (rightly measured) is proportional to the impressed force (rightly measured).

5. *No* à posteriori *proof of the general ultimate physical truth (or of this more special truth it includes) is possible; because every supposed process of verification assumes it.*

These, cleared from entanglements, are the theses held by me, and defended in the following pages.

APPENDIX A.

(TO REPLIES TO CRITICISMS.)

LETTERS, AND PARTS OF LETTERS PUBLISHED IN *NATURE*,

Between March 26 and June 18, 1874; here reproduced (with notes) as further elucidating the doctrine respecting physical axioms, set forth in the foregoing "Replies to Criticisms."

(From *Nature*, April 16, 1874.)

ABSENCE from town has delayed what further remarks I have to make respecting the disputed origin of physical axioms.

The particular physical axiom in connection with which the general question was raised, was the Second Law of Motion. It stands in the *Principia* as follows :—

"*The alteration of motion is ever proportional to the motive force impressed; and is made in the direction of the right line in which that force is impressed.*

"If any force generates a motion, a double force will generate double the motion, a triple force triple the motion, whether that force be impressed altogether and at once, or gradually and successively. And this motion (being always directed the same way with the generating force), if the body moved before, is added to or subducted from the former motion, according as they directly conspire with or are directly contrary to each other; or obliquely joined, when they are oblique, so as to produce a new motion compounded from the determination of both."

As this, like each of the other Laws of Motion, is called an

axiom;* as the paragraph appended to it is simply an amplification, or re-statement in a more concrete form; as there are no facts named as bases of induction, nor any justifying experiment; and as Newton proceeds forthwith to draw deductions; it was a legitimate inference that he regarded this truth as *à priori.* My statement to this effect was based on the contents of the *Principia* itself; and I think I was warranted in assuming that the nature of the Laws of Motion, as conceived by Newton, was to be thence inferred.

The passages quoted by the *British Quarterly* Reviewer from Newton's correspondence, which were unknown to me, show that this was not Newton's conception of them. Thus far, then, my opponent has the best of the argument. Several qualifying considerations have to be set down, however.

(1) Clearly, the statements contained in the *Principia* do not convey Newton's conception; otherwise there would have been no need for his explanations. The passages quoted prove that he wished to exclude these cardinal truths from the class of hypotheses, which he said he did not make; and to do this he had to define them.

(2) By calling them "axioms," and by yet describing them as principles "*deduced* from phenomena," he makes it manifest that he gives the word "axiom" a sense widely unlike the sense in which it is usually accepted.

(3) Further, the quotations fail to warrant the statement that the Laws of Motion are proved true by the truth of the *Principia.* For if the fulfilment of astronomical predictions made in pursuance of the *Principia,* is held to be the evidence "on which they chiefly rest to this day," then, until thus justified, they are unquestionably hypotheses. Yet Newton says they are not hypotheses.

Newton's view may be found without seeking for it in his

* It is true that in Newton's time, "axiom" had not the same rigorously defined meaning as now; but it suffices for my argument that, standing unproved as a basis for physical deductions, it bears just the same relation to them that a mathematical axiom does to mathematical deductions.

letters: it is contained in the *Principia* itself. The scholium to Corollary VI. begins thus:—

"Hitherto I have laid down such principles as have been received by mathematicians, and are *confirmed* by abundance of experiments. By the two first Laws and the two first Corollaries, Galileo discovered that the descent of bodies observed the duplicate ratio of the time, and that the motion of projectiles was in the curve of a parabola; experience *agreeing* with both," &c.

Now as this passage precedes the deductions constituting the *Principia*, it shows conclusively, in the first place, that Newton did not think "the whole of the *Principia* was the proof" of the Laws of Motion, though the Reviewer asserts that it is. Further, by the words I have italicised, Newton implicitly describes Galileo as having asserted these Laws of Motion, if not as gratuitous hypotheses (which he says they are not), then as *à priori* intuitions. For a proposition which is *confirmed* by experiment, and which is said to *agree* with experience, must have been entertained before the alleged verifications could be reached. And as before he made his experiments on falling bodies and projectiles, Galileo had no facts serving as an inductive basis for the Second Law of Motion, the law could not have been arrived at by induction.

Let me end what I have to say on this vexed question by adding a further reason to those I have already given, for saying that physical axioms cannot be established experimentally. The belief in their experimental establishment rests on the tacit assumption that experiments can be made, and conclusions drawn from them, without any truths being postulated. It is forgotten that there is a foundation of preconceptions without which the perceptions and inferences of the physicist cannot stand—*preconceptions which are the products of simpler experiences than those yielded by consciously-made experiments.* Passing over the many which do not immediately concern us, I will name only that which does,—the exact quantitative relation [of proportionality] between cause and effect. It is taken by the chemist as a truth needing no proof, that if two volumes of hydrogen unite with one volume of oxygen to form a certain

quantity of water, four volumes of hydrogen uniting with two volumes of oxygen will form double the quantity of water. If a cubic foot of ice at 32° is liquefied by a specified quantity of heat, it is taken to be unquestionable that three times the quantity of heat will liquefy three cubic feet. And similarly with mechanical forces, the unhesitating assumption is that if one unit of force acting in a given direction produces a certain result, two units will produce twice the result. Every process of measurement in a physical experiment takes this for granted; as we see in one of the simplest of them—the process of weighing. If a measured quantity of metal, gravitating towards the Earth, counterbalances a quantity of some other substance, the truth postulated in every act of weighing is, that any multiple of such weight will counterbalance an equi-multiple of such substance. That is to say, each unit of force is assumed to work its equivalent of effect in the direction in which it acts. Now this is nothing else than the assumption which the Second Law of Motion expresses in respect to effects of another kind. "If any force generates a motion, a double force will generate a double motion," &c., &c.; and when carried on to the composition of motions, the law is, similarly, the assertion that any other force, acting in any other direction, will similarly produce in that direction a proportionate motion. So that the law simply asserts the exact equivalence [or proportionality] of causes and effects of this particular class, while all physical experiments *assume* this exact equivalence [or proportionality] among causes and effects of all classes. Hence, the proposal to prove the Laws of Motion experimentally, is the proposal to make a wider assumption for the purpose of justifying one of the narrower assumptions included in it.

Reduced to its briefest form, the argument is this:—If definite quantitative relations [of proportionality] between causes and effects be assumed *à priori*, then, the Second Law of Motion is an immediate corollary. If there are not definite quantitative relations [of proportionality] between causes and effects, all the conclusions drawn from physical experiments are invalid. And

further, in the absence of this *à priori* assumption of equivalence, the quantified conclusion from any experiment may be denied, and any other quantification of the conclusion asserted.*

HERBERT SPENCER.

Entire misconstruction of the view expressed above, having been shown by a new assailant, who announced himself as also "A Senior Wrangler," Mr. James Collier wrote on my behalf an explanatory letter, published in *Nature* for May 21, from which the following passages are extracts:—

"The cue may be taken from an experience described in Mr. Spencer's *Principles of Psychology* (§ 468, note), where it is shown that when with one hand we pull the other, we have in the feeling of tension produced in the limb pulled, a measure of the reaction that is equivalent to the action of the other limb. Both terms of the relation of cause and effect are in this case present to consciousness as muscular tensions, which are our symbols of forces in general. While no motion is produced they are felt to be equal, so far as the sensations can serve to measure equality; and when excess of tension is felt in the one arm, motion is experienced in the other. Here, as in the examples about to be given, the relation between cause and effect, though numerically indefinite, is definite in the respect that every additional increment of cause produces an additional increment of effect; and it is out of this and similar experiences that the idea of the relation of proportionality grows and becomes organic.

"A child, when biting its food, discovers that the harder he bites the deeper is the indentation; in other words, that the

* The above letter, written after absence at Easter had involved a week's delay, and written somewhat hurriedly to prevent the delay of a second week, was less carefully revised than it should have been. The words in square brackets, obviously implied by the reasoning, and specifically implied by the illustrations, were not in the letter as originally published.

more force applied, the greater the effect. If he tears an object with his teeth, he finds that the more he pulls the more the thing yields. Let him press against something soft, as his own person, or his clothes, or a lump of clay, and he sees that the part or object pressed yields little or much, according to the amount of the muscular strain. He can bend a stick the more completely the more force he applies. Any elastic object, as a piece of india-rubber or a catapult, can be stretched the farther the harder he pulls. If he tries to push a small body, there is little resistance and it is easy to move; but he finds that a big body presents greater resistance and is harder to move. The experience is precisely similar if he attempts to lift a big body and a little one; or if he raises a limb, with or without any object attached to it. He throws a stone: if it is light, little exertion propels it a considerable distance; if very heavy, great exertion only a short distance. So, also, if he jumps, a slight effort raises him to a short height, a greater effort to a greater height. By blowing with his mouth he sees that he can move small objects, or the surface of his morning's milk, gently or violently according as the blast is weak or strong. And it is the same with sounds: with a slight strain on the vocal organs he produces a murmur; with great strain he can raise a shout.

"The experiences these propositions record all implicate the same consciousness—the notion of proportionality between force applied and result produced; and it is out of this latent consciousness that the axiom of the perfect quantitative equivalence of the relations between cause and effect is evolved. To show how rigorous, how irreversible, this consciousness becomes, take a boy and suggest to him the following statements:—Can he not break a string he has, by pulling? tell him to double it, and then he will break it. He cannot bend or break a particular stick: let him make less effort and he will succeed. He is unable to raise a heavy weight: tell him he errs by using too much force. He can't push over a small chest: he will find it easier to upset a larger one. By blowing hard he cannot move a given object: if he blows lightly, he will move it. By great exertion

he cannot make himself audible at a distance: but he will make himself heard with less exertion at a greater distance. Tell him to do all or any of these, and of course he fails. The propositions are unthinkable, and their unthinkableness shows that the consciousness which yields them is irreversible. These, then, are preconceptions, properly so called, which have grown unconsciously out of the earliest experiences, beginning with those of the sucking infant, are perpetually confirmed by fresh experiences, and have at last become organized in the mental structure.

* * * * *

"Mr. Spencer's argument appears to be briefly this:—1. There are numberless experiences unconsciously acquired and unconsciously accumulated during the early life of the individual (in harmony with the acquisitions of all ancestral individuals) which yield the preconception, long anteceding anything like conscious physical experiments, that physical causes and effects vary together quantitatively. This is gained from all orders of physical experiences, and forms a universal preconception respecting them, which the physicist or other man of Science brings with him to his experiments.

"2. Mr. Spencer showed in three cases—chemical, physical, and mechanical—that this preconception, so brought, was tacitly involved in the conception which the experimenter drew from the results of his experiments.

"3. Having indicated this universal preconception, and illustrated its presence in these special conceptions, Mr. Spencer goes on to say that it is involved also in the special conception of the relation between force and motion, as formulated in the 'Second Law of Motion.' He asserts that this is simply one case out of the numberless cases in which all these consciously-reasoned conclusions rest upon the unconsciously-formed conclusions that precede reasoning. Mr. Spencer alleges that as it has become impossible for a boy to think that by a smaller effort he can jump higher, and for a shopman to think that smaller weights will outbalance greater quantities, and for the

physicist to think that he will get increased effects from diminished causes, so it is impossible to think that 'alteration of motion' is not 'proportional to the motive force impressed.' And he maintains that this is, in fact, a latent implication of unconsciously-organized experiences just as much as those which the experimenter necessarily postulates."

To meet further misinterpretations, a second letter was written by Mr. Collier and published in *Nature* for June 4. The following are passages from it:—

"Having but limited space, and assuming that the requisite qualifications would be made by unbiased readers, I passed over all those details of the child's experiences which would have been required in a full exposition. Of course I was aware that in the bending of a stick the visible effect does not increase in the same ratio as the force applied; and hardly needed the 'Senior Wrangler' to tell me that the resistance to a body moving through a fluid increases in a higher ratio than the velocity. It was taken for granted that he, and those who think with him, would see that out of all these experiences, in some of which the causes and effects are simple, and in others of which they are complex, there grows the consciousness that the proportionality is the more distinct the simpler the antecedents and consequents. This is part of the preconception which the physicist brings with him and acts upon. Perhaps it is within the 'Senior Wrangler's' knowledge of physical exploration, that when the physicist finds a result not bearing that ratio to its assigned cause which the two were ascertained in other cases to have, he immediately assumes the presence of some perturbing cause or causes, which modify the ratio. There is, in fact, no physical determination made by any experimenter which does not assume, as an *à priori* necessity, that there cannot be a deviation from proportion without the presence of such additional cause.

"Returning to the general issue, perhaps the 'Senior Wrangler' will pay some respect to the judgment of one who was a Senior Wrangler too, and a great deal more—who was distinguished not only as a mathematician but as an astronomer, a physicist, and also as an inquirer into the methods of science: I mean Sir John Herschel. In his *Discourse on the Study of Natural Philosophy*, he says:—

"'When we would lay down general rules for finding and facilitating our search, among a great mass of assembled facts, for their common cause, we must have regard to the characters of that relation which we intend by cause and effect.'

"Of these 'characters' he sets down the third and fourth in the following terms:—

"'Increase or diminution of the effect, with the increased or diminished intensity of the cause, in cases which admit of increase and diminution.'

"'Proportionality of the effect to its cause in all cases of *direct unimpeded* action.'

"Observe that, in Sir J. Herschel's view, these are 'characters' of the relation of cause and effect to be accepted as 'general rules for *guiding* and facilitating our search' among physical phenomena—truths that must be taken for granted *before* the search, not truths derived *from* the search. Clearly, the 'proportionality of the effect to its cause in all cases of direct and unimpeded action' is here taken as *à priori*. Sir J. Herschel would, therefore, have asserted, with Mr. Spencer, that the Second Law of Motion is *à priori*; since this is one of the cases of the 'proportionality of the effect to its cause.'

"And now let the 'Senior Wrangler' do what Sir J. Herschel has not done or thought of doing—*prove* the proportionality of cause and effect. Neither he, nor any other of Mr. Spencer's opponents, has made the smallest attempt to deal with this main issue. Mr. Spencer alleges that this cognition of proportionality is *à priori*: not in the old sense, but in the sense that it grows out of experiences that precede reasoning. His opponents, following Prof. Tait in the assertion that Physics is a purely experimental science, containing, therefore, no *à priori*

truths, affirm that this cognition is *à posteriori*—a product of conscious induction. Let us hear what are the experiments. It is required to establish the truth that there is proportionality between causes and effects, *by a process which nowhere assumes* that if one unit of force produces a certain unit of effect, two units of such force will produce two units of such effect. Until the 'Senior Wrangler' has done this he has left Mr. Spencer's position untouched."

APPENDIX B.

(TO REPLIES TO CRITICISMS.)

FURTHER CORRESPONDENCE.

[After publication of the letters from which the foregoing are reproduced, there appeared in *Nature* certain rejoinders containing misrepresentations even more extreme than those preceding them. There resulted a direct correspondence with two of the writers—Mr. Robert B. Hayward, of Harrow, and Mr. J. F. Moulton, my original assailant, the author of the article in the *British Quarterly Review*. This correspondence, in which I demanded from these gentlemen the justifications for their statements, formed part of this Appendix in its pamphlet form, as distributed among those who are competent to judge of the questions at issue. It is needless to give permanence to the replies and rejoinders. The character of Mr. Moulton's allegations, quite congruous with those I have exposed in the "Replies to Criticisms," may be inferred from one of the sentences closing my reply—"Wonderful to relate, my inductive proof that proportionality [of cause and effect] is taken for granted, he cites as my inductive proof of proportionality itself!" The result of the interchange of letters with Mr. Hayward, was to make it clear that "the thing I assert is not really disputed; and the thing disputed, I have nowhere asserted." While, however, the controversial part of the correspondence may fitly disappear, I retain an expository part embodied in the following letter to Mr. Hayward.]

38, Queen's Gardens, Bayswater,
June 21st, 1874.

Sir,—Herewith I send you a copy of your letter with my interposed comments. I think those comments will make it clear to you that I have not committed myself to three different definitions of our consciousness of the Second Law of Motion.

As others may still feel a difficulty, such as you seem to have

felt, in understanding that which familiarity has made me regard as simple, I will endeavour, by a synthetic exposition, to make clear the way in which these later and more complex products of organized experiences stand related to earlier and simpler products. To make this exposition easier to follow, I will take first our Space-consciousness and the derived conceptions.

On the hypothesis of Evolution, the Space-consciousness results from organized motor, tactual, and visual experiences. In the *Principles of Psychology*, §§ 326—346, I have described in detail what I conceive to have been its genesis. Such Space-consciousness so generated, is one possessed in greater or less degree by all creatures of any intelligence; becoming wider, and more definite, according to the degree of mental evolution which converse with the environment has produced. How deeply registered the external relations have become in the internal structure, is shown by the facts that the decapitated frog pushes away with one or both legs the scalpel applied to the hind part of its body, and that the chick, as soon as it has recovered from the exhaustion of escaping from the egg, performs correctly-guided actions (accompanied by consciousness of distance and direction) in picking up grains. Ascending at once to such organized and inherited Space-consciousness as exists in the child, and which from moment to moment it is making more complete by its own experiences (aiding the development of its nervous system into the finished type of the adult, by the same exercises that similarly aid the development of its muscular system), we have to observe that, along with increasingly-definite ideas of distance and direction, it gains unawares certain more special ideas of geometrical relations. Take one group of these. Every time it spreads open its fingers it sees increase of the angles between them, going along with increase of the distances between the finger-tips. In opening wide apart its own legs, and in seeing others walk, it has continually before it the relation between increase or decrease of base in a triangle having equal sides, and increase or decrease

of the angle included by those sides. [The relation impressed on it being simply that of *concomitant variation:* I do not speak of any more definite relation, which, indeed, is unthinkable by the young.] It does not observe these facts in such way as to be conscious that it has observed them; but they are so impressed upon it as to establish a rigid association between certain mental states. Various of its activities disclose space-relations of this class more definitely. The drawing of a bow exhibits them in another way and with somewhat greater precision; and when, instead of the ends of a bow, capable of approaching one another, the points of attachment are fixed and the string elastic, the connexion between increasing length in the sides of an isosceles triangle and increasing acuteness of the included angle, is still more forced upon the attention; though it still does not rise into a conscious cognition. This is what I mean by an "unconsciously-formed preconception." When, in course of time, the child, growing into the boy, draws diagrams on paper, and, among other things, draws isosceles triangles, the truth that, the base being the same, the angle at the apex becomes more acute as the sides lengthen, is still more definitely displayed to him; and when his attention is drawn to this relation he finds that he cannot think of it as being otherwise. If he imagines the lengths of the sides to change, he cannot exclude the consciousness of the correlative change in the angle; and presently, when his mental power is sufficiently developed, he perceives that if he continues to lengthen the sides in imagination, the lines approach parallelism as the angle approaches zero: yielding a conception of the relations of parallel lines. Here the consciousness has risen into the stage of definite conception. But, manifestly, the definite conception so reached is but a finishing of the preconceptions previously reached, and would have been impossible in their absence; and these unconsciously-formed preconceptions would similarly have been impossible in the absence of the still earlier consciousnesses of distance, direction, relative position, embodied in the consciousness of Space. The whole evolution

truths, affirm that this cognition is *à posteriori*—a product of conscious induction. Let us hear what are the experiments. It is required to establish the truth that there is proportionality between causes and effects, *by a process which nowhere assumes* that if one unit of force produces a certain unit of effect, two units of such force will produce two units of such effect. Until the 'Senior Wrangler' has done this he has left Mr. Spencer's position untouched."

between greatness of quantity in the two, and between smallness of quantity in the two; later still, a tacit assumption of proportionality, though without a distinct consciousness that the assumption has been made; and, finally, a rising of this assumption into definite recognition, as a truth necessarily holding where the forces are simple. Throughout its life every creature has, *within the actions of its moving parts,* forces and motions conforming to the Laws of Motion. If it has a nervous system, the differences among the muscular tensions and the movements initiated, register themselves in a vague way in that nervous system. As the nervous system develops, along with more developed limbs, there are at once more numerous different experiences * * * of momentum generated, of connected actions and reactions (as when an animal tears the food which it holds with its paws); and, at the same time, there are, in its more developed nervous system, increased powers of appreciating and registering these differences. All the resulting connexions in consciousness, though unknowingly formed and unknowingly entertained, are

ing side. The parts of this stand in different relations of distance from the subtended angle; and as the line is lengthened, each added part differs from the preceding parts in its distance from the angle. That is to say, one set of simple directly-connected geometrical relations, is here involved with another set; and the relation between the side and the angle is such that the law of relative increase involves the co-operation of two sets of factors. Now the distinguishing the true proportionality (between the angle and the arc) from the relation which simulates proportionality (between the angle and the side) is just that process of final development of exact conceptions, which I assert to be the finishing step of all the preceding development; and to be impossible in its absence. And the truth to which my assailants shut their eyes, is that, just as among these conceptions of space-relations, the conception of exact proportionality can be reached only by evolution from the crude notion of proportionality, formed before reasoning begins; so, among the force-relations, the conception of proportionality finally reached, when simple causes and their effects are disentagled by analytical intelligence, can be reached only by evolution of the crude notion of proportionality, established as a preconception by early experiences which reinforce ancestral experiences.

ever present as guides to action: witness the proportion between the effort an animal makes and the distance it means to spring; or witness the delicate adjustments of muscular strains to changes of motion, made by a swallow catching flies or a hawk swooping on its quarry. Manifestly, then, these experiences, organized during the earlier stages of mental evolution, form a body of consciousness, not formulated into cognitions, not present even as preconceptions, but nevertheless present as a mass of associations *in which the truths of relation between force and motion are potentially present.* On ascending to human beings of the uncultured sort, we reach a stage at which some nascent generalization of these experiences occurs. The savage has not expressed to himself the truth that if he wants to propel his spear further he must use more force; nor does the rustic put into a distinct thought the truth that to raise double the weight he must put forth twice the effort; but in each there is a tacit assumption to this effect, as becomes manifest on calling it in question. So that, in respect of these and other simple mechanical actions, there exist unconsciously-formed preconceptions. And just as the geometrical truths presented in a rude way by the relations among surrounding objects, are not overtly recognized until there is some familiarity with straight lines, and diagrams made of them; so, until linear measures, long used, have led to the equal-armed lever, or scales, and thus to the notion of equal units of force, this mechanical preconception cannot rise into definiteness. Nor after it has risen into definiteness does it for a long time reach the form of a consciously-held cognition; for neither the village huxter nor the more cultivated druggist in the town, recognizes the general abstract truth that, when uninterfered with, equi-multiples of causes and their effects are necessarily connected. But now observe that this truth, acted upon with more or less distinct consciousness of it by the man of science, and perfected by him through analysis and abstraction, is thus perfected only as the last step in its evolution. This definite cognition is but the finished form of a consciousness long in preparation—a con-

sciousness the body of which is present in the brute, takes some shape in the primitive man, reaches greater definiteness in the semi-civilized, becomes afterwards an assumption distinct though not formulated, and takes its final development only as it rises into a consciously-accepted axiom. Just as there is a continuous evolution of the nervous system, so is there a continuous evolution of the consciousness accompanying its action; just as the one grows in volume, complexity, and definiteness, so does the other; and just as necessary as the earlier stages are to the later in the one case, are they in the other. To suppose that the finished conceptions of science can exist without the unfinished common knowledge which precedes them, or this without still earlier mental acquisitions, is the same thing as to suppose that we can have the correct judgments of the adult without passing through the crude judgments of the youth, the narrow, incoherent ones of the child, and the vague, feeble ones of the infant. So far is it from being true that the view of physical axioms held by me, is one which bases cognitions on some other source than experience, it asserts experience to be the only possible source of these, as of other cognitions; but it asserts, further, that not simply is the consciously-acquired experience of the present needful, but that *for the very possibility of gaining it* we are indebted to the accumulation of all past experiences. Not I, but my antagonists, are really chargeable with accepting the ancient *à priori* view; since, without any explanation of them or justification of them, they posit as unquestionable the assumptions underlying every experiment and the conclusion drawn from it. The belief in physical causation, assumed from moment to moment as necessary in every experiment and in all reasoning from it, is a belief which, if not justified by the hypothesis above set forth, is tacitly asserted as an *à priori* belief. Contrariwise, my own position is one which affiliates all such beliefs upon experiences acquired during the whole past; which alleges those experiences as the only warrant for them; which asserts that during the converse between the mind and its environment, necessary

connexions in Thought, such as those concerning Space, have resulted from infinite experiences of corresponding necessary connexions in Things; and that, similarly, out of perpetual converse with the Forces manifested to us in Space, there has been a progressive establishment of internal relations answering to external relations, in such wise that there finally emerge as physical axioms, certain necessities of Thought which answer to necessities in Things.

I need scarcely say that I have taken the trouble of making my comments on your letter, and of writing this further exposition, with a view to their ulterior use.

I am, &c.,

HERBERT SPENCER.

APPENDIX C.

(TO REPLIES TO CRITICISMS.)

SUMMARY OF RESULTS

THOSE who deny a general doctrine enunciated by Mayer as the basis of his reasonings, habitually assumed by Faraday as a guiding principle in drawing his conclusions, distinctly held by Helmholtz, and tacitly implied by Sir John Herschel—those, I say, who deny this general doctrine and even deride it, should be prepared with clear and strong reasons for doing this. Having been attacked, not in the most temperate manner, for enunciating this doctrine and its necessary implications in a specific form, I have demanded such reasons. Observe the responses to the demand.

1. The *British Quarterly* Reviewer quoted for my instruction the *dictum* of Professor Tait, that "Natural Philosophy is an experimental, and not an intuitive science. No *à priori* reasoning can conduct us demonstratively to a single physical truth." Thereupon I inquired what Professor Tait meant "by speaking of 'physical *axioms*,' and by saying that the cultured are enabled 'to see *at once* their *necessary* truth?'" .... **No reply.**

2. Instead of an answer to the question, how this intuition of necessity can be

alleged by Professor Tait consistently with his other doctrine, the Reviewer quotes, as though it disposed of my question, Professor Tait's statement that "as the properties of matter might have been such as to render a totally different set of laws axiomatic, *these laws* [of motion] *must be considered as resting on convictions drawn from observation and experiment, and not on intuitive perception.*" Whereupon I inquired how Professor Tait knows that "the properties of matter *might have been*" other than they are. I asked how it happened that his intuition concerning things *as they are not*, is so certain that, by inference from it, he discredits our intuitions concerning things *as they are* ....

No reply: Professor Tait told, *à propos* of my question, a story of which no one could discover the application; but, otherwise, declined to answer. Nor was any answer given by his disciple.

3. Further, I asked how it happened that Professor Tait accepted as bases for Physics, Newton's Laws of Motion; which were illustrated but not *proved* by Newton, and of which no *proofs* are supplied by Professor Tait, in the *Treatise on Natural Philosophy.* I went on to examine what conceivable *à posteriori* warrant there could be if there

was no warrant *à priori;* and I pointed out that neither from terrestrial nor from celestial phenomena could the First Law of Motion be deduced without a *petitio principii*.......................................

No reply: the Reviewer characterized my reasoning as "utterly erroneous" (therein differing entirely from two eminent authorities who read it in proof); but beyond so characterizing it he said nothing.

4. To my assertion that Newton gave no proof of the Laws of Motion, the Reviewer rejoined that "the whole of the *Principia* was the proof." On which my comment was that Newton called them "axioms," and that axioms are not commonly supposed to be proved by deductions from them ....

The Reviewer quotes from one of Newton's letters a passage showing that though he called the Laws of Motion "axioms," he regarded them as principles "made general by induction;" and that therefore he

could not have regarded them as *à priori.*

5. In rejoinder, I pointed out that whatever conception Newton may have had of these "axioms," he explicitly and distinctly excluded them from the class of "hypotheses." Hence I inferred that he did not regard the whole of the *Principia* as the proof, which the Reviewer says it is; since an assumption made at the outset, to be afterwards justified by the results of assuming it, is an "hypothesis"..............

No reply.

6. Authority aside, I examined on its merits the assertion that the Laws of Motion are, or can be, proved true by the ascertained truth of astronomical predictions; and showed that the process of verification itself assumed those Laws......

No reply.

7. To make still clearer the fact that ultimate physical truths are, and must be, accepted as *à priori*, I pointed out that in every experiment the physicist tacitly assumes a relation between cause and effect, such that, if one unit of cause produces its unit of effect, two units of the cause will produce two units of the effect; and I argued that this general assumption included the special assumption asserted in the Second Law of Motion ....

No reply: that is to say, no endeavour to show the untruth of this statement, but a

quibble based on my omission of the word "proportionality" in places where it was implied, though not stated.

8. Attention was drawn to a passage from Sir John Herschel's *Discourse on the Study of Natural Philosophy*, in which the "proportionality of the effect to its cause in all cases of *direct unimpeded* action" is included by him among "the characters of that relation which we intend by cause and effect;" and in which this assumption of proportionality is set down as one preceding physical exploration, and not as one to be established by it .... No reply.

9. Lastly, a challenge to prove this proportionality. "It is required to establish the truth that there is proportionality between causes and effects, *by a process which nowhere assumes* that if one unit of force produces a certain unit of effect, two units of such force will produce two units of such effect."........................... No reply.

Thus on all these essential points my three mathematical opponents allow judgment to go against them by default. The attention of readers has been drawn off from the main issues by the discussion of side issues. Fundamental questions have been evaded, and new questions of subordinate kinds raised.

What is the implication? One who is able to reach and to carry the central position of his antagonist, does not spend

his strength on small outposts. If he declines to assault the stronghold, it must be because he sees it to be impregnable.

The trouble I have taken to meet criticisms and dissipate misapprehensions, I have taken because the attack made on the special doctrine defended, is part of an attack on the ultimate doctrine underlying the deductive part of *First Principles*—the doctrine that the quantity of existence is unchangeable. I agree with Sir W. Hamilton that our consciousness of the necessity of causation, results from the impossibility of conceiving the totality of Being to increase or decrease. The proportionality of cause and effect is an implication: denial of it involves the assertion that some quantity of cause has disappeared without effect, or some quantity of effect has arisen without cause. I have asserted the *à priori* character of the Second Law of Motion, *under the abstract form in which it is expressed*, simply because this, too, is an implication, somewhat more remote, of the same ultimate truth. And my sole reason for insisting on the validity of these intuitions, is that, on the hypothesis of Evolution, absolute uniformities in things have produced absolute uniformities in thoughts; and that necessary thoughts represent infinitely-larger accumulations of experiences than the observations, experiments, and reasonings of any single life.

XII.

TRANSCENDENTAL PHYSIOLOGY.

[FROM THE NATIONAL REVIEW OF OCTOBER, 1857.]

TRANSCENDENTAL PHYSIOLOGY.*

In Mathematics, the Transcendental Analysis is one which, passing beyond those particular relations of numbers dealt with by arithmetic, and passing beyond those general numerical relations which form the subject-matter of ordinary algebra, concerns itself with the still higher generalities underlying these general relations. The title Transcendental Anatomy is used to distinguish that division of biological science which treats, not of the structure of individual organisms, but of the general principles of structure common to vast and varied groups of organisms,—the unity of plan discernible throughout multitudinous genera and orders which are widely different in appearance. And here, under the head of Transcendental Physiology, we purpose putting together sundry laws of development and function which apply, not to particular kinds or classes of organisms, but to

* This essay, which was first published in the *National Review* for 1857, under the title of "The Ultimate Laws of Physiology," was contained in the first series of reprinted essays, published in December of that year. From the American reprint of the first and second series of essays, re-arranged, it was omitted. The English edition being out of print, and the American edition alone current here, this essay is, and has been for these ten years, inaccessible. I now add it to this second edition of the third series, because, besides presenting in their original forms certain of the general views afterwards elaborated in *First Principles*, it contains some other general views of importance.

all organisms: laws, some of which have not, we believe, been hitherto enunciated.

By way of unobtrusively introducing the general reader to this highest class of biological truths, let us begin by briefly noticing one or two with which he is already familiar. Take first, the relation between the activity of an organ and its growth. This is a universal relation. It holds, not only of a bone, a muscle, a nerve, an organ of sense, a mental faculty; but of every gland, every viscus, every element of the body. It is seen, not in man only, but in each animal in which we have adequate opportunity of tracing it; and not in animals only, but in plants. Always providing that the performance of function is not so excessive as to produce disorder, or exceed the repairing powers either of the system at large or of the particular agencies by which nutriment is brought to the organ—always providing this, it is a law of organized bodies that, other things equal, development varies as function. On this law are based all maxims and methods of right education, intellectual, moral, and physical; and when statesmen are wise enough to see it, this law will be found to underlie all right legislation.

Another of these truths which are co-extensive with the organic creation, is that of hereditary transmission. It is not, as commonly supposed, that hereditary transmission is exemplified merely in the perpetuation of the family peculiarities seen either in immediate or remote progenitors. Nor does the law of hereditary transmission comprehend only such more general facts as that modified plants or animals become the parents of permanent varieties; and that new kinds of wheat or potatoes, new breeds of sheep or cattle, new races of men, have been thus originated. These are but minor

exemplifications of the law. Understood in its entirety, the law is, that each plant or animal produces others of like kind with itself: the likeness of kind consisting not so much in the repetition of individual traits as in the assumption of the same generic structure. This truth has become by daily illustration so familiar as almost to have lost its significance. That wheat produces wheat, —that existing oxen are descended from ancestral oxen, —that every unfolding organism ultimately takes the form of the class, order, genus, and species from which it sprang; is a fact which, by force of repetition, has assumed in our minds almost the character of a necessity. It is in this, however, that the law of hereditary transmission is principally displayed: the phenomena commonly referred to it being quite subordinate manifestations. And the law, as thus understood, is universal. Not forgetting the apparent, but only apparent, exceptions presented by the strange class of phenomena known as "alternate generation," the truth that like produces like is common to all races of organisms.

Let us take next a universal physiological law of a less conspicuous kind; and one of but recent establishment. To the ordinary observer, it seems that the multiplication of organisms proceeds in a variety of ways. He sees that the young of the higher animals are born with a general likeness to their parents; that birds lay eggs, which they foster and hatch; that fish deposit spawn and leave it. Among plants, he finds that while in some cases new individuals grow from seeds only, in others, as in that of the potato, they also grow from tubers; that by certain plants layers are sent out, take root, and develop new individuals; and that many plants are produced from cuttings and buds. Further,

in the mould that makes its appearance on stale food, and the infusoria that soon swarm in water exposed to air and light, he sees a mode of generation which, seeming, as it does, inexplicable, he is apt to consider "spontaneous." The reader of popular science thinks the modes of reproduction still more various. He discovers that whole tribes of creatures multiply by gemmation—by a development from the body of the parent of buds which, after unfolding into the parental form, separate and lead independent lives. He learns that among the microscopic forms of both animal and vegetable life, the ordinary mode of multiplication is by spontaneous fission—by a splitting-up of the original individual into two or more individuals, which by and by severally repeat the process. Still more remarkable are the cases in which, as in the *Aphis*, an egg gives rise to an imperfect female, from which other imperfect females are born viviparously, grow, and in their turns bear other imperfect females; and so on for eight, ten, or more generations, until finally, perfect males and females are viviparously produced. But now under all these, and many more, modified modes of multiplication, the advanced physiologist finds that there is at bottom complete uniformity. The starting-point, not only of every higher animal or plant, but of every clan of organisms which by fission or gemmation have sprung from a single organism, is always a spore, seed, or ovum. The millions of infusoria or of aphides which, by subdivision or gemmation, have proceeded from one individual; the countless plants that have been successively propagated from one original plant by cuttings or tubers; are, in common with the highest creature, primarily descended from a fertilised germ. And in all cases—in the humblest alga as in the oak, in the protozoon as in the

mammal—this fertilised germ results from the union of the contents of two cells. Whether, as in the lowest forms of life, these two cells are of seemingly identical nature; or whether, as in higher forms, they are distinguishable into sperm-cell and germ-cell; it remains throughout true that from their combination results the mass out of which is evolved a new organism or new series of organisms. That this law is without exception we are not prepared to say; for in the case of the *Aphis* certain experiments seem to imply that under special conditions the descendants of an original individual may continue multiplying for ever, without further fecundation; and it may be so in other cases. But we know of no case in Nature where it *actually is* so; for although there are certain plants whose seeds have never yet been seen, it is more probable that our observations are in fault than that these plants are exceptions. And until we find undoubted exceptions the above stated induction must stand. Here, then, we have another of the truths of Transcendental Physiology: a truth which, so far as we know, *transcends* all distinctions of genus, order, class, kingdom, and applies to every living thing whatever.

Yet another generalisation of like universality is that which formulates the process of organic development. To the uninitiated this seems variable. No obvious parallelism exists between the unfolding of a plant and the unfolding of an animal. There is no manifest similarity between the development of a mammal, which proceeds without break from its first to its last phase, and that of an insect, which is divided into strongly-marked stages—egg, larva, pupa, imago. Nevertheless it is now an established fact, that all organisms are evolved after one general method. At the outset the germ of every plant or animal is homogeneous; and

every advance towards maturity is an advance towards greater heterogeneity. Every organized thing commences as an almost structureless mass, and progresses towards its ultimate complexity by the establishment of distinctions upon distinctions,—by the divergence of tissues from tissues and organs from organs. Here, then, we have yet another biological law of transcendental generality.

Having thus indicated the scope of Transcendental Physiology by presenting its leading truths, we have prepared the way for the considerations that are to follow.

And first, returning to the last of the great generalisations above given, let us inquire more nearly how this change from the homogeneous to the heterogeneous is carried on. Usually it is said to result from successive differentiations. This, however, we conceive to be an incomplete account of the process. As every physiologist knows, there occurs, during the evolution of an organism, not only separation of parts, but coalescence of parts. There is not only segregation, but aggregation. The heart, at first a large, long, pulsating blood-vessel, by and by twists upon itself and becomes integrated. The layer of bile-cells constituting the rudimentary liver, do not simply diverge from the surface of the intestine on which they at first lie; but they simultaneously consolidate into a definite organ. And the gradual concentration seen in these and other cases forms a part of the developmental process : a part which, though more or less recognized by Milne-Edwards and others, does not seem to have been included as an essential element in the conception of the developmental process.

This progressive integration, which is seen alike in

tracing up the several stages passed through by every embryo, and in ascending from the lower organic forms to the higher, may be most conveniently studied under several heads. Let us consider first what may be called *longitudinal integration.*

The lower *Annulosa*—worms, myriapods, &c.—are characterized by the great number of segments of which they consist, reaching in some cases to several hundreds; but as we advance to the higher *Annulosa*—centipedes, crustaceans, insects, spiders—we find this number greatly reduced, down to twenty-two, thirteen, and even fewer; and accompanying this there is a shortening or integration of the whole body, reaching its extreme in crab and spider. Similarly if we watch the development of an individual crustacean or insect. The thorax of a lobster, which, in the adult, forms, with the head, one compact box containing the viscera, is made up by the union of a number of segments which in the embryo were separable. The thirteen distinct divisions seen in the body of a caterpillar, become further integrated in the butterfly: several segments are consolidated to form the thorax, and the abdominal segments are more aggregated than they originally were. The like truth is seen when we pass to the internal organs. In the inferior annulose forms, and in the larvæ of the higher ones, the alimentary canal consists either of a tube that is uniform from end to end, or else bulges into a succession of stomachs, one to each segment; but in the developed forms there is a single well-defined stomach. In the nervous, vascular, and respiratory systems a parallel concentration may be traced. Again, in the development of the *Vertebrata* we have sundry examples of longitudinal integration. The coalescence of several segments to form the skull is one instance of it. It is further illustrated

in the *os coccygis*, which results from the fusion of a number of caudal vertebræ. And in the consolidation of the sacral vertebræ of a bird it is also well exemplified.

That which we may distinguish as *transverse integration*, is clearly illustrated among the *Annulosa* in the development of the nervous system. Leaving out those most degraded forms which do not present distinct ganglia, it is to be observed that the lower annulose animals, in common with the larvæ of the higher, are severally characterized by a double chain of ganglia running from end to end of the body; while in the more perfectly formed annulose animals this double chain becomes more or less completely united into a single chain. Mr. Newport has described the course of this concentration as exhibited in insects; and by Rathke it has been traced in the crustaceans. In the early stages of the *Astacus fluviatilis*, or common cray-fish, there is a pair of separate ganglia to each ring. Of the fourteen pairs belonging to the head and thorax, the three pairs in advance of the mouth consolidate into one mass to form the brain, or cephalic ganglion. Meanwhile out of the remainder, the first six pairs severally unite in the median line, while the rest remain more or less separate. Of these six double ganglia thus formed, the anterior four coalesce into one mass; the remaining two coalesce into another mass; and then these two masses coalesce into one. Here we see longitudinal and transverse integration going on simultaneously; and in the highest crustaceans they are both carried still further. The *Vertebrata* clearly exhibit this transverse integration in the development of the generative system. The lowest of the mammalia—the *Monotremata*—in common with birds, to which they are in many respects allied, have oviducts which towards their lower extremities are di-

lated into cavities, severally performing in an imperfect way the function of a uterus. "In the *Marsupialia* there is a closer approximation of the two lateral sets of organs on the median line; for the oviducts converge towards one another and meet (without coalescing) on the median line; so that their uterine dilatations are in contact with each other, forming a true 'double uterus.' . . . In many of the *Rodentia* the uterus still remains divided into two lateral halves; whilst in others these coalesce at their lower portions, forming a rudiment of the true 'body' of the uterus in the human subject. This part increases at the expense of the lateral 'cornua' in the higher herbivora and carnivora; but even in the lower quadrumana the uterus is somewhat cleft at its summit."* And this process of transverse integration, which is still more striking when observed in all its details, is accompanied by parallel, though less important, changes in the opposite sex. Once more, in the increasing commissural connection of the cerebral hemispheres, which, though separate in the lower vertebrata, become gradually more united in the higher, we have another instance. And further ones of a different order, but of like general implication, are supplied by the vascular system.

Now it seems to us that the various forms of integration here exemplified, which are commonly set down as so many independent phenomena, ought to be generalized, and included in the formula describing the process of development. The fact that in an adult crab, numerous pairs of ganglia originally separate have become fused into a single mass, is a fact only second in significance to the differentiation of its alimentary canal into stomach and intestine. That in the higher *Annulosa*,

* Carpenter's Prin. of Comp. Phys. p. 617.

a single heart replaces the string of rudimentary hearts constituting the dorsal blood-vessel in the lower *Annulosa*, (reaching in one species to the number of one hundred and sixty,) is a truth as much needing to be comprised in the history of evolution, as is the formation of a respiratory surface by an involution of the skin. A right conception of the genesis of a vertebral column, includes not only the differentiations from which result the *chorda dorsalis* and the vertebral segments imbedded in it; but quite as much, or more, it includes the coalescence of numerous vertebral processes with their respective vertebral bodies. The changes in virtue of which several things become one, demand recognition equally with those in virtue of which one thing becomes several. Evidently, then, the current statement which ascribes the developmental progress to differentiations alone, is incomplete. Adequately to express the facts, we must say that the transition from the homogeneous to the heterogeneous is carried on by differentiations and subordinate integrations.

It may not be amiss here to ask—What is the meaning of these integrations? The evidence seems to show that it is in some way dependent upon community of function. The eight segments which coalesce to make the head of a centipede, have the common purpose of protecting the cephalic ganglia, and affording a solid fulcrum for the jaws, &c.; as also have the many bones which unite to form a vertebrate's skull. In the consolidation of the several pieces which constitute a mammalian pelvis, and in the anchylosis of from ten to nineteen vertebræ in the sacrum of a bird, we have kindred instances of the integration of parts which transfer the weight of the body to the legs. The more or less complete fusion of the tibia with the fibula and the radius

with the ulna in the ungulated mammals, whose habits do not require any rotation of the limbs, is a fact of like meaning. And all the instances lately given—the concentration of ganglia, the replacement of many pulsating blood-sacs by fewer and finally by one, the fusion of two uteri into a single uterus—have the same implication. Whether, as in some cases, the integration is a mere consequence of the continued growth which eventually brings into contact adjacent parts performing similar duties; or whether, as in other cases, there is an actual approximation of these parts before their union; or whether, as in yet other cases, the integration is of that indirect kind which arises when, out of a number of like organs, one, or a group, discharges an ever-increasing share of the common function, and so grows while the rest dwindle and disappear;—the general fact remains the same, that there is a tendency to the unification of parts having similar duties.

The tendency, however, has limiting conditions; the recognition of which will explain some apparent exceptions. Let us take instances. In the human fœtus, as in the lower vertebrata, the eyes are placed one on each side of the head. In the process of evolution they become relatively nearer, and at birth are in front; though they are still, in the European infant as in the adult savage, proportionately further apart than they afterwards become. But this approximation shows no signs of further increase Two reasons for this suggest themselves. Inasmuch as the eyes, being directed to the same object, have a common function, they tend to become one; but inasmuch as they are directed to different sides of the same object, and so have different functions, they tend to remain two; and possibly their ultimate positions depend on the balance of these

opposing tendencies. A more probable explanation however seems to be, that the interposed structures do not permit any nearer approach. For the orbits of the eyes to be brought closer together, would imply a decrease in the size of the olfactory chambers; and as these are probably not larger than is demanded by their present functional activity, no decrease can take place. Again, if we trace up the external organs of smell through fishes,* reptiles, ungulate mammals and unguiculate mammals, to man, we perceive a general tendency to coalescence in the median line; and on comparing the savage with the civilized, or the infant with the adult, we see this approach of the nostrils carried furthest in the most perfect of the species. But since the septum which divides them has the function both of an evaporating surface for the lachrymal secretion, and a ramifying surface for a nerve ancillary to that of smell, it does not disappear entirely: the integration remains incomplete. These and other like instances do not however militate against the hypothesis. They merely show that the tendency is sometimes antagonized by other tendencies. Bearing in mind which qualification, we may say, that as differentiation of parts is connected with difference of function, so there appears to be a connection between integration of parts and sameness of function.

Intimately related to the general truth that the evolution of all organisms is carried on by combined differentiations and integrations, is another general truth which physiologists appear not to have recognized. When we

* With the exception, perhaps, of the Myxinoid fishes, in which what is considered as the nasal orifice is single, and on the median line. But seeing how unusual is the position of this orifice, it seems questionable whether it is the true homologue of the nostrils.

look at the organic creation in its *ensemble*, we may observe that, on passing from lower to higher forms, we pass to forms which are not only characterized by a greater differentiation of parts, but are at the same time more widely differentiated from the surrounding medium. This truth may be contemplated under various aspects.

In the first place, it is illustrated in *structure*. The advance from the homogeneous to the heterogeneous, itself involves an increasing distinction from the inorganic world. In the lowest *Protozoa*, as that mere speck of jelly the *Protogenes*, we have a homogeneity approaching to that of water, or earth; and the ascent to organisms of greater and greater complexity of structure, is an ascent to organisms that are in that respect more strongly contrasted with the structureless environment.

In *form*, again, we see the same fact. A general characteristic of inorganic matter is its indefiniteness of form; and this is also a characteristic of the lower organisms as compared with the higher. Speaking generally, plants are less definite than animals, both in shape and size—admit of greater modifications from variations of position and nutrition. Among animals, the *Amœba* and its allies are not only structureless but amorphous: the form is never specific, and is constantly changing. Of the organisms resulting from the aggregation of amœba-like creatures, we find that while some, as the *Foraminifera*, assume a certain definiteness of form, in their shells at least; others, as the Sponges, are very irregular. In the Zoophytes and in the *Polyzoa*, we see compound organisms, most of which have a mode of growth not more determinate than that of plants. Among the higher animals, however, not only is the

mature shape of each species very definite, but the individuals of each species differ little in size.

A parallel increase of contrast is likewise seen in *chemical composition.* With but few exceptions, and those only partial ones, the lowest animal and vegetal forms are inhabitants of the water; and water is almost their sole constituent. Desiccated *Protophyta* and *Protozoa* shrink into mere dust; and among the Acalephes there are but a few grains of solid matter to a pound of water. The higher aquatic plants, in common with the higher aquatic animals, possessing as they do much greater tenacity of substance, also contain a greater proportion of the organic elements; and so are chemically more unlike their medium. And when we pass to the superior classes of organisms—land plants and animals—we find that, chemically considered, they have little in common either with the earth on which they stand or the air which surrounds them.

In *specific gravity,* too, the like is seen. The simplest forms, in common with the spores and gemmules of higher ones, are as nearly as may be of the same specific gravity as the water in which they float; and though it cannot be said that among aquatic creatures superior specific gravity is a standard of general superiority, yet we may fairly say that the superior orders of them, when divested of the appliances by which their specific gravity is regulated, differ more from water in their relative weight than do the lowest. In terrestrial organisms the contrast becomes extremely marked. Trees and plants, in common with insects, reptiles, mammals, birds, are all of a specific gravity considerably less than the earth and immensely greater than the air.

Yet further, we see the law similarly fulfilled in respect of *temperature.* Plants generate but an extremely small

quantity of heat, which is to be detected only by very delicate experiments; and practically they may be considered as having the same temperature as their environment. The temperature of aquatic animals is but little above that of the surrounding water: that of the invertebrata being mostly less than a degree above it, and that of fishes not exceeding it by more than two or three degrees, save in the case of some large red-blooded fishes, as the tunny, which exceed it by nearly ten degrees. Among insects, the range is from two to ten degrees above that of the air: the excess varying according to their activity. The heat of reptiles is from four to fifteen degrees more than the heat of their medium. While mammals and birds maintain a heat which continues almost unaffected by external variations, and is often greater than that of the air by seventy, eighty, ninety, and even a hundred degrees.

Once more, in greater *self-mobility* a progressive differentiation is traceable. The especial characteristic by which we distinguish dead matter is its inertness: some form of independent motion is our most general test of life. Passing over the indefinite border-land between the animal and vegetal kingdoms, we may roughly class plants as organisms which, while they exhibit that species of motion implied in growth, are not only devoid of locomotive power, but with some unimportant exceptions are devoid of the power of moving their parts in relation to each other; and thus are less differentiated from the inorganic world than animals. Though in those microscopic *Protophyta* and *Protozoa* inhabiting the water—the spores of algæ, the gemmules of sponges, and the infusoria generally—we see locomotion produced by ciliary action; yet this locomotion, while rapid relatively to their size, is absolutely slow. Of the

Cœlenterata, a great part are either permanently rooted or habitually stationary; and so have scarcely any self-mobility but that implied in the relative movements of parts; while the rest, of which the common jelly-fish will serve as a sample, have mostly but little ability to move themselves through the water. Among the higher aquatic *Invertebrata*,—cuttle-fishes and lobsters, for instance,—there is a very considerable power of locomotion; and the aquatic *Vertebrata* are, considered as a class, much more active in their movements than the other inhabitants of the water. But it is only when we come to air-breathing creatures that we find the vital characteristic of self-mobility manifested in the highest degree. Flying insects, mammals, birds, travel with a velocity far exceeding that attained by any of the lower classes of animals; and so are more strongly contrasted with their inert environment.

Thus, on contemplating the various grades of organisms in their ascending order, we find them more and more distinguished from their inanimate media in *structure*, in *form*, in *chemical composition*, in *specific gravity*, in *temperature*, in *self-mobility*. It is true that this generalization does not hold with complete regularity. Organisms which are in some respects the most strongly contrasted with the environing inorganic world, are in other respects less so than inferior organisms. As a class, mammals are higher than birds; and yet they are of lower temperature, and have smaller powers of locomotion. The stationary oyster is of higher organization than the free-swimming medusa; and the cold-blooded and less heterogeneous cod, is quicker in its movements than the warm-blooded and more heterogeneous sloth. But the admission that the several aspects under which this increasing contrast shows itself bear a variable ratio

to each other, does not conflict with the general truth enunciated. Contemplating the facts in the mass, it cannot be denied that the successively higher grades of organisms are severally characterized, not only by a greater differentiation of parts, but also by a greater differentiation from the surrounding medium in sundry other physical attributes. It would seem that this peculiarity has some necessary connection with superior vital manifestations. One of those lowly gelatinous forms, so transparent and colourless as to be with difficulty distinguished from the water it floats in, is not more like its medium in chemical, mechanical, optical, thermal, and other properties, than it is like in the passivity with which it submits to all the actions brought to bear upon it; while the mammal does not more widely differ from inanimate things in these properties, than it differs in the activity with which it meets surrounding changes by compensating changes in itself. And between these two extremes, we shall observe a constant ratio between these two kinds of contrast. Whence we may say, that in proportion as an organism is physically like its environment it remains a passive partaker of the changes going on in its environment; while in proportion as it is endowed with powers of counteracting such changes, it exhibits greater unlikeness to its environment.

Thus far we have treated our subject inductively, in conformity with established usage; but much may be done in this and other departments of biologic inquiry by pursuing the deductive method. The generalizations at present constituting the science of physiology, both general and special, have been reached *à posteriori;* but certain fundamental data have now been discovered, starting

from which, we may reason our way *à priori*, not only to some of the truths that have been ascertained by observation and experiment, but also to some others. The possibility of such *à priori* conclusions will be at once recognized on considering a few familiar cases.

Physiologists and chemists have shown that a necessary condition to vital activity in animals is oxidation. The oxygen requisite for this is contained in the surrounding medium—air or water, as the case may be. If the organism be some minute protozoon, mere contact of its external surface with the oxygenated medium secures the requisite oxidation; but if the organism is bulky, and so exposes a surface that is small in proportion to its mass, any considerable oxidation cannot be thus secured. One of two things is therefore implied. Either this bulky organism, receiving no oxygen but that absorbed through its integument, must possess but little vital activity; or else, if it possesses much vital activity, there must be some extensive ramified surface, internal or external, through which adequate aeration may take place—a respiratory apparatus. That is to say, lungs, or branchiæ, or their equivalents, are predicable *à priori* as possessed by all active creatures of any size.

Similarly with respect to nutriment. There are *entozoa* which, living in the insides of other animals and being constantly bathed by nutritive fluids, absorb a sufficiency through their outer surfaces; and so have no need of stomachs and do not possess them. But all other animals, inhabiting media that are not in themselves nutritive, but only contain masses of food here and there, must have appliances by which these masses of food may be utilized. Evidently mere external contact of a solid organism with a solid portion of nutriment, could not result in the assimilation of it in any moderate time,

if at all. To achieve this end, there must be both a solvent or macerating action, and an extended surface fit for containing and imbibing the dissolved products: that is, there must be a digestive cavity. Thus, given the ordinary conditions of animal life, and the possession of stomachs by all creatures living under these conditions may be deductively known.

Carrying out the train of reasoning still further, we may infer the existence of a vascular system, or something equivalent to it, in all creatures of any size and activity. In a comparatively small inert animal, such as the hydra, which consists of little more than a sac having a double wall—an outer layer of cells forming the skin, and an inner layer forming the assimilating surface—there is no need for a special apparatus to diffuse through the body the absorbed aliment; for the body is little more than a wrapper to the food it encloses. But where the bulk is considerable, or where the activity is such as to involve much waste and repair, or where both these characteristics exist, there is a manifest necessity for a system of blood-vessels. It is not enough that there be adequately extensive surfaces for assimilation and aeration; for in the absence of any means of conveyance the absorbed elements can be of little or no use to the organism at large. Evidently there must be channels of communication. When, as in the *Medusæ*, we find these channels of communication consisting simply of branching canals opening out of the stomach and spreading through the disk; we may know, *à priori*, that such creatures are comparatively inactive: seeing that the nutriment thus partially distributed throughout their bodies is crude and dilute, and that there is no efficient appliance for keeping it in motion. Conversely, when we meet with a creature of considerable size which

displays much vivacity, we may know, *à priori*, that it must have an apparatus for the unceasing supply of concentrated nutriment, and of oxygen, to every organ —a pulsating vascular system.

It is manifest, then, that setting out from certain known fundamental conditions to vital activity, we may deduce from them sundry of the chief characteristics of organized bodies. Doubtless these known fundamental conditions have been inductively established. But in this they do not differ from the ground truths of deductive science in general; all of which are inductions. What we wish to show is, that, given these inductively-established primary facts in physiology, we may with safety draw certain general deductions from them. And, indeed, the legitimacy of such deductions, though not formally acknowledged, is practically recognized in the convictions of every physiologist; as may be readily proved by citing a few illustrations. Thus, were a physiologist to find a creature exhibiting complex and variously co-ordinated movements, and yet having no nervous system; he would be less astonished at the breach of his empirical generalization that all such creatures have nervous systems, than at the disproof of his unconscious deduction that all creatures exhibiting complex and variously co-ordinated movements must have an "internuncial" apparatus by which the co-ordination may be effected. Or were he to find a creature having a rapid circulation and a rapid respiration, but yet showing a low temperature, the proof so afforded that active change of matter was not, as he had inferred from chemical data, the cause of animal heat, would stagger him more than would the exception to the constantly-observed relation between these characteristics. Clearly, then, the *à priori* method already plays

a part in physiological reasoning: if not ostensibly employed as a means of reaching new truths, it is at least privately appealed to for confirmation of truths reached *à posteriori*.

We think, however, that the illustrations above given go far to show that it may to a considerable extent be safely used as an independent instrument of research. The necessities for a nutritive system, a respiratory system, and a vascular system, in all animals of size and vivacity, seem to us legitimately inferable from the conditions to continued vital activity. Given the physical and chemical data, and these structural peculiarities may be deduced with as much certainty as may the hollowness of an iron ball from its power of floating in water.

Let us not, however, be understood as supposing that the more *special* physiological truths can be deductively reached. Our argument by no means implies this. Legitimate deduction presupposes adequate data; and in respect to all the *special* phenomena of organic growth, structure, and function, adequate data are unattainable, and will probably ever remain so. It is only in the case of the more *general* physiological truths, such as those above instanced, where we have something like adequate data, that deductive reasoning becomes possible.

And here we arrive at the point to which the foregoing considerations are introductory. We propose now to show that there are certain still more general attributes of organized bodies, which are deducible from certain still more general attributes of things.

In an essay on "Progress: its Law and Cause," elsewhere published,* we have endeavoured to show that

* In the *Westminster Review* for April, 1857; and reprinted in the first volume of Essays, &c.

the transformation of the homogeneous into the heterogeneous, in which all progress, organic or other, essentially consists, is consequent on the production of many effects by one cause—many changes by one force. Having pointed out that this is a law of all things, we proceeded to show deductively that the multiform evolutions of the homogeneous into the heterogeneous—astronomic, geologic, ethnologic, social &c., were explicable as corollaries of this law. And though in the case of organic evolution, lack of data disabled us from specifically tracing out the progressive complications as due to the multiplication of effects ; yet, we found sundry indirect evidences that it was so. Now in so far as this conclusion, that organic evolution results from the decomposition of each expended force into several forces, was inferred from the general law previously pointed out, it was an example of deductive physiology. The particular was concluded from the universal.

We here propose in the first place to show, that there is another general truth standing in immediate correlation with the above ; and in common with it underlying all forms of progress, and therefore the progress of organisms—a truth which may indeed be considered as taking precedence of it in respect of time, if not in respect of generality. This truth is, that *the condition of homogeneity is a condition of unstable equilibrium.*

The phrase *unstable equilibrium* is one used in mechanics to express a balance of forces of such kind, that the interference of any further force, however minute, will destroy the arrangement previously subsisting, and bring about a totally different arrangement. Thus, a stick poised on its lower end is in unstable equilibrium : however exactly it may be placed in a perpendicular position, as soon as it is left to itself it begins, at first

imperceptibly, to lean on one side, and with increasing rapidity falls into another attitude. Conversely, a stick suspended from its upper end is in stable equilibrium: however much disturbed, it will return to the same position. Our meaning is, then, that the state of homogeneity, like the state of the stick poised on its lower end, is one that cannot be maintained; and that hence inevitably results the first step in its gravitation towards the heterogeneous. Let us take a few illustrations.

Of mechanical ones the most familiar is that of the scales. If they be accurately made and not clogged by dirt or rust, it is impossible to keep a pair of scales perfectly balanced: eventually one scale will descend and the other ascend—they will assume a heterogeneous relation. Again, if we sprinkle over the surface of a fluid a number of equal-sized particles, having an attraction for each other, they will, no matter how uniformly distributed, by and by concentrate irregularly into one or more groups. Were it possible to bring a mass of water into a state of perfect homogeneity—a state of complete quiescence, and exactly equal density throughout—yet the radiation of heat from neighbouring bodies, by affecting differently its different parts, would inevitably produce inequalities of density and consequent currents; and would so render it to that extent heterogeneous. Take a piece of red-hot matter, and however evenly heated it may at first be, it will quickly cease to be so: the exterior cooling faster than the interior, will become different in temperature from it. And the lapse into heterogeneity of temperature, so obvious in this extreme case, takes place more or less in all cases. The action of chemical forces supplies other illustrations. Expose a fragment of metal to air or water, and in course of time it will be coated with a film of

oxide, carbonate, or other compound: that is, its outer parts will become unlike its inner parts. In short, every homogeneous aggregration of matter tends to lose its balance in some way or other—either mechanically, chemically, thermally, or electrically; and the rapidity with which it lapses into a non-homogeneous state is simply a question of time and circumstances. Social bodies illustrate the law with like constancy. Endow the members of a community with equal properties, positions, powers, and they will forthwith begin to slide into inequalities. Be it in a representative assembly, a railway board, or a private partnership, the homogeneity, though it may continue in name, inevitably disappears in reality.

The instability thus variously illustrated becomes still more manifest if we consider its rationale. It is consequent on the fact that the several parts of any homogeneous aggregate are necessarily exposed to different forces—forces that differ either in kind or amount; and being exposed to different forces they are of necessity differently modified. The relations of outside and inside, and of comparative nearness to neighbouring sources of influence, imply the reception of influences that are unlike in quantity or quality or both; and as a corollary from the law of "the conservation of force," it follows that unlike changes will be produced in the parts thus dissimilarly acted upon. Thus the unstable equilibrium of any homogeneous aggregate can be shown both inductively and deductively.

And now let us consider the bearing of this general truth on the evolution of organisms. The germ of a plant or animal is one of these homogeneous aggregates whose equilibrium is unstable. But it has not simply the ordinary instability of homogeneous aggre-

gations: it has something more. For it consists of units that are themselves specially characterized by instability. The constituent atoms of organic matter are distinguished by the feebleness of the affinities which hold their component elements together: they are extremely sensitive to heat, light, electricity, and the chemical actions of foreign elements; that is—they are peculiarly liable to be modified by disturbing forces. Hence then it follows, *à priori*, that a homogeneous aggregation of these unstable atoms will have an excessive tendency to lose its equilibrium. It will have a quite special aptitude to lapse into a non-homogeneous state. It will rapidly gravitate towards heterogeneity.

Moreover, the process must repeat itself in each of the subordinate groups of organic units that are differentiated by the modifying forces. Each of these subordinate groups, like the original group, must gradually, in obedience to the influences acting upon it, lose its balance of parts—must pass from a uniform into a multiform state. And so on continuously.

Thus, starting from the general laws of things, and the known chemical attributes of organic matter, we may conclude deductively that the homogeneous germs of organisms have a peculiar proclivity towards a non-homogeneous state; which may be either the state we call decomposition or the state we call organization.

Thus far our reasoning brings us to a conclusion only of the most general nature. We merely find that *some* kind of heterogeneity is inevitable; but as yet there is nothing to tell us *what* kind. Besides that *orderly* heterogeneity which distinguishes organisms, there is the *disorderly* or *chaotic* heterogeneity, into which a loose mass of inorganic matter lapses; and at present no

reason has been given why the homogeneous germ of a plant or animal should not lapse into the disorderly instead of the orderly heterogeneity. Let us see whether some light may not be thrown on this point by pursuing still further the line of argument hitherto followed.

We have seen that the instability of homogeneous aggregations in general, and of organic ones in particular, is consequent on the different ways and degrees in which their constituent parts are exposed to the disturbing forces brought to bear on them: they are differently acted upon, and therefore become different. Manifestly, then, a rationale of the special changes which a germ undergoes, must be sought in the particular relations which its several parts bear to each other and to their environment. However it may be masked, we may suspect the fundamental principle of organization to be, that the many like units forming a germ acquire those kinds and degrees of unlikeness which their respective positions entail. But let us speak more specifically.

Take a mass of unorganized but organizable matter—either the body of one of the lowest living forms, or the germ of one of the higher. Consider its circumstances. It is immersed in water or air; or it is contained within a parent organism. Wherever placed, however, its outer and inner parts stand differently related to surrounding agencies—nutriment, oxygen, and the various stimuli. But this is not all. Whether it lies quiescent at the bottom of the water or on the leaf of a plant; whether it moves through the water preserving some definite attitude; or whether it is in the inside of an adult; it equally results that certain parts of its surface are more exposed to surrounding agencies than other parts—in some cases more exposed to light, heat,

or oxygen, and in others to the maternal tissues and their contents. The destruction of its original equilibrium is therefore certain. It may take place in one of two ways. Either the disturbing forces may be such as to overbalance the affinities of the organic elements, in which case there results that chaotic heterogeneity known as decomposition; or, as is ordinarily the case, such changes are induced as do not destroy the organic compounds, but only modify them: the parts most exposed to the modifying forces being most modified. Hence there result those first differentiations which constitute incipient organization. From the point of view thus reached, suppose we look at a few cases: neglecting for the present all consideration of the tendency to assume the hereditary type.

Note first what appear to be exceptions, as the *Amœba*. In this creature and its allies, the substance of the jelly-like body remains throughout life unorganized—undergoes no permanent differentiations. But this fact, which seems directly opposed to our inference, is really one of the most significant evidences of its truth. For what is the peculiarity of this division of the *Protozoa?* Its members undergo perpetual and irregular changes of form—they show no persistent relations of parts. What lately was a portion of the interior is now protruded, and, as a temporary limb, is attached to some object it happens to touch. What is now a part of the surface will presently be drawn, along with the atom of nutriment sticking to it, into the centre of the mass. Thus there is an unceasing interchange of places; and the relations of inner and outer have no permanent existence. But by the hypothesis, it is only in virtue of their unlike positions with respect to modifying forces, that the originally like units of a living mass become

unlike. We must not therefore expect any established differentiations of parts in creatures which exhibit no established differences of position in their parts.

This negative evidence is borne out by abundant positive evidence. When we turn from these proteiform specks of living jelly to organisms having an unchanging distribution of substance, we find differences of tissue corresponding to differences of relative position. In all the higher *Protozoa*, as also in the *Protophyta*, we meet with a fundamental differentiation into cell-membrane and cell-contents, answering to that fundamental contrast of conditions implied by the terms outside and inside. And on passing from what are roughly classed as unicellular organisms to the lowest of those which consist of aggregated cells, we equally observe the connection between structural differences and differences of circumstances. In the Sponge, permeated throughout by currents of sea-water, the absence of definite organization corresponds with the absence of definite unlikeness of conditions. But in the *Thalassicolla*—a transparent, colourless, body found floating passively at the surface of the sea, and consisting essentially of "a mass of cells united by jelly"—there is displayed a rude structure obviously subordinated to the primary relations of centre and surface: in all of its many and important varieties, the parts exhibit more or less of concentric arrangement.

After this primary modification, by which the outer tissues are differentiated from the inner, the next in order of constancy and importance is that by which some part of the outer tissues is differentiated from the rest; and this corresponds with the almost universal fact that some part of the outer tissues is more exposed to certain environing influences than the rest. Here, as

before, the apparent exceptions are extremely significant. Some of the lowest vegetal organisms, as the *Hematococci* and *Protococci*, evenly imbedded in a mass of mucus, or dispersed through the Arctic snow, display no differentiations of surface; the several parts of the surface being subjected to no definite contrast of conditions. The *Thalassicolla* above mentioned, floating unfixed in the water, and passively rolled about by the waves, successively presents all its sides to the same agencies; and all its sides are alike. Ciliated spheres like the *Volvox* have no parts of their periphery unlike other parts; and it is not to be expected that they should have; seeing that as they revolve in all directions, they do not, in traversing the water, permanently expose any part to special conditions. But when we come to creatures that are either fixed, or while moving preserve definite attitudes, we no longer find uniformity of surface. The gemmule of a Zoophyte, which during its locomotive stage is distinguishable only into outer and inner tissues, no sooner takes root than its upper end begins to assume a different structure from its lower. The free-swimming embryo of an aquatic annelid, being ovate and not ciliated all over, moves with one end foremost; and its differentiations proceed in conformity with this contrast of circumstances.

The principle thus displayed in the humbler forms of life is visible in the development of the higher; though being here soon masked by the assumption of the hereditary type, it cannot be traced far. It is, however, conspicuous in those first stages of the higher organisms during which they simulate the lowest Thus the "mulberry mass" of cells into which a fertilized vertebrate ovum first resolves itself, soon begins to exhibit a difference between the outer and inner parts, answering

to the fundamental difference of circumstances. The peripheral cells, after reaching a more complete development than the interior ones, coalesce into a membrane inclosing the rest; and the cells lying next to these outer ones become aggregrated with them, and increase the thickness of the germinal membrane, while the central cells liquefy. Again, one part of the germinal membrane presently becomes distinguishable as the germinal spot; and without asserting that the cause of this is to be found in the unlike relations which the respective parts of the germinal membrane bear to environing influences, it is clear that we have in these unlike relations an element of disturbance tending to destroy the original homogeneity of the germinal membrane. Further, the germinal membrane by and by divides into two layers, internal and external; the one in contact with the liquefied part of the yolk, the other exposed to the surrounding fluids: this contrast of circumstances being in obvious correspondence with the contrast of structure which follows it. Once more, the subsequent appearance of the vascular layer between these mucous and serous layers, as they have been named, admits of a like interpretation. And in this and the various complications that now begin to show themselves, we may see coming into play that general law of the multiplication of effects flowing from one cause, to which the increase of heterogeneity was elsewhere ascribed:* which multiplication of effects is, indeed, co-operative with the action here described; seeing that each newly differentiated part becomes the centre of a new influence acting upon all other parts in different degrees.

Confining our remarks as we do to the most general facts of development, we think that some light is thus

* See Essay on "Progress: its Law and Cause."

thrown on them. That the unstable equilibrium of a homogeneous germ must be destroyed by the unlike exposure of its several units to surrounding influences, is an *à priori* conclusion. And it seems also to be an *à priori* conclusion, that the several units thus differently acted upon, must either be decomposed, or must undergo such modifications of nature as may enable them to live in the respective circumstances they are thrown into. In other words—*they must become adapted to their conditions.* Indeed, we might almost infer as much without going through the foregoing train of reasoning. The superficial organic units (be they the outer cells of a "mulberry mass," or be they the outer molecules of an individual cell) must assume the function which their position necessitates; and assuming this function, must acquire such character as the performance of it involves. The layer of organic units lying in contact with the yolk must be those through which the yolk is assimilated; and so must be adapted to the assimilative office. On this condition only does the process of organization appear possible. We might almost say that just as some original race of animals, which multiplies and spreads into different regions of the earth, becomes differentiated into several races through the adaptation of each to its conditions of life; so, the originally-homogeneous population of cells arising in a fertilized germ-cell, becomes divided into several populations of cells that grow unlike in virtue of the unlikenesses of their circumstances.

Moreover, it is to be remarked in further proof of our position, that it finds its clearest and most abundant illustrations where the conditions of the case are the simplest and most general—where the phenomena are the least involved: we mean in the production of individual cells. The structures which presently arise round

nuclei in a blastema, and which have in some way been determined by those nuclei as centres of influence, evidently conform to the law; for the parts of the blastema in contact with the nuclei are differently circumstanced from the parts not in contact with them. Again, the formation of a membrane round each of the masses of granules into which the endochrome of an alga-cell breaks up, is an instance of analogous kind. And should the recently-asserted fact that cells may arise round vacuolæ in a mass of organizable substance, be confirmed, another good example will be furnished; for such portions of substance as bound these vacant spaces are subject to influences unlike those to which other portions of the substance are subject. If, then, we can thus most clearly trace this law of modification in these primordial processes, as well as in those more complex but analogous ones exhibited in the early changes of an ovum, we have strong reason for thinking that the law is a fundamental one.

But, as already more than once hinted, this principle, understood in the simple form here presented, supplies no key to the detailed phenomena of organic development. It fails entirely to explain generic and specific peculiarities; and indeed leaves us equally in the dark respecting those more important distinctions by which families and orders are marked out. Why two ova, similarly exposed in the same pool, should become the one a fish, and the other a reptile, it cannot tell us. That from two different eggs placed under the same hen, should respectively come forth a duckling and a chicken, is a fact not to be accounted for on the hypothesis above developed. We have here no alternative but to fall back upon the unexplained principle of hereditary transmission. The capacity possessed by an unorganized

germ of unfolding into a complex adult which repeats ancestral traits in the minutest details, and that even when it has been placed in conditions unlike those of its ancestors, is a capacity impossible for us to understand. That a microscopic portion of seemingly-structureless matter, should embody an influence of such kind that the resulting man will in fifty years after become gouty or insane, is a truth which would be incredible were it not daily illustrated. But though the *manner* in which hereditary likeness, in all its complications, is conveyed, is a mystery passing comprehension, it is quite conceivable that it is conveyed in subordination to the law of adaptation above explained; and we are not without reasons for thinking that it is so. That acquired peculiarities resulting from the adaptation of constitution to conditions, are transmissible to offspring, is an established fact. Such acquired peculiarities consist of differences of structure or composition in one or more of the tissues. That is to say, out of the aggregation of similar organic units composing a germ, the group going to the formation of a particular tissue, will take on the special character which the adaptation of that tissue to new circumstances had produced in the parents. We know this to be a general law of organic modifications. Further, it is the *only* law of organic modifications of which we have any evidence. It is not impossible then that it is the universal law; comprehending not simply those minor modifications which offspring inherit from recent ancestry, but comprehending also those major modifications distinctive of species, genus, order, class, which they inherit from antecedent races of organisms. And thus it *may be* that the law of adaptation is the sole law; applying not only to the differentiation of any race of organisms into several races, but also to the differen-

tiations of the race of organic units composing a germ, into the many races of organic units composing an adult. So understood, the process gone through by every unfolding organism will consist, partly in the direct adaptation of its elements to their several circumstances, and partly in the assumption of characters resulting from analogous adaptations of the elements of all ancestral organisms.

But our argument does not commit us to any such far-reaching speculation as this; which we introduce simply as suggested by it, not involved. All we are here concerned to show, is, that the deductive method aids us in interpreting some of the more general phenomena of development; and this we think we have shown. That all homogeneous aggregations are in unstable equilibrium is a universal truth, from which is deducible the instability of every organic germ. From the known sensitiveness of organic compounds to chemical, thermal, and other disturbing forces, we further infer the *unusual* instability of every organic germ—a proneness far beyond that of other homogeneous aggregations to lapse into a heterogeneous state. And by the same line of reasoning we are led to the additional inference, that the first divisions into which a germ resolves itself, being severally in states of unstable equilibrium, are similarly prone to undergo further changes; and so on continuously. Moreover, we have found it to be equally an *à priori* conclusion that as, in all other cases, the loss of homogeneity is due to the different amounts and kinds of force brought to bear upon the different parts; so, in this case also, difference of circumstances is the primary cause of differentiation. Add to which, that as the several changes undergone by the respective parts thus differently acted upon, are

changes which do not destroy their vital activity, they must be changes which bring that vital activity into subordination to the incident forces—they must be adaptations; and the like must be in some sense true of all the subsequent changes. Thus by deductive reasoning we get considerable insight into the method of organization. However unable we are, and probably ever shall be, to comprehend the way in which a germ is made to take on the special form of its race, we may yet comprehend the general principles which regulate its first modifications; and remembering the unity of plan so conspicuous throughout Nature, we may *suspect* that these principles regulate all succeeding modifications.

A controversy now going on among zoologists, opens yet another field for the application of the deductive method. We believe that the question whether there does or does not exist a *necessary correlation* among the several parts of any organism, is determinable *à priori*.

Cuvier, who first asserted this necessary correlation, professed to base his restorations of extinct animals upon it. Geoffroy St. Hilaire and De Blainville from different points of view, contested Cuvier's hypothesis; and the discussion, which has much interest as bearing on palæontology, has been recently revived under a somewhat modified form: Professors Huxley and Owen being respectively the assailant and defender of the hypothesis.

Cuvier says—"Comparative anatomy possesses a principle whose just development is sufficient to dissipate all difficulties; it is that of the correlation of forms in organized beings, by means of which every kind of organized being might, strictly speaking, be recognized by a fragment of any of its parts. Every organized

being constitutes a whole, a single and complete system, whose parts mutually correspond and concur by their reciprocal reaction to the same definite end. None of these parts can be changed without affecting the others; and consequently each taken separately, indicates and gives all the rest." And he then cites sundry illustrations: arguing that the carnivorous form of tooth necessitating a certain action of the jaw, implies a particular form in its condyle; implies also limbs fit for seizing and holding prey; and therefore implies claws, a certain structure of the leg-bones, a certain form of shoulder-blade; and winds up by saying, that "the claw, the scapula, the condyle, the femur, and all the other bones, taken separately, will give the tooth or one another; and by commencing with any one, he who had a rational conception of the laws of the organic economy could reconstruct the whole animal."

It will be seen, that the method of restoration here contended for, is based upon the alleged physiological necessity of the connection between these several peculiarities. The argument used is, not that a scapula of a certain shape may be recognized as having belonged to a carnivorous mammal because we always find that carnivorous mammals *do* possess such scapulas; but because they *must* possess them—because carnivorous habits would be impossible without them. And in the above quotation Cuvier asserts that the necessary correlation which he considers so obvious in these cases, exists between all parts of the system: admitting, however, that in consequence of our limited knowledge of physiology, we are unable in many cases to trace this necessary correlation, and are obliged to base our conclusions upon observed coexistences, of which we do not understand the reason, but which we find invariable.

Now Professor Huxley has recently shown that, in the first place, this empirical method, which Cuvier introduces as quite subordinate, and to be used only in aid of the rational method, is really the method which Cuvier habitually employed—the so-called rational method remaining practically a dead letter; and, in the second place, he has shown that Cuvier himself has in several places so far admitted the inapplicability of the rational method, as virtually to surrender it as a method. But more than this, Professor Huxley contends that the alleged law of necessary correlation is not true. Quite admitting the physiological dependence of parts on each other, he denies that it is a dependence of a kind that could not be otherwise. "Thus the teeth of a lion and the stomach of the animal are in such relation that the one is fitted to digest the food which the other can tear; they are physiologically correlated; but we have no reason for affirming this to be a necessary physiological correlation, in the sense that no other could equally fit its possessor for living on recent flesh. The number and form of the teeth might have been quite different from that which we know them to be, and the construction of the stomach might have been greatly altered; and yet the functions of these organs might have been equally well performed."

Thus much is needed to give our readers an idea of the controversy as it now stands. It is not here our purpose to go more at length into the evidence cited on both sides; we simply wish to show that the question may be settled deductively. Before going on to do this, however, we must briefly notice two collateral points.

In his defence of the Cuvierian doctrine, Professor Owen avails himself of the *odium theologicum*. He attributes to his opponents "the insinuation and masked

advocacy of the doctrine subversive of a recognition of the Higher Mind." Now, saying nothing about the questionable propriety of thus prejudging a point in science, we think this is a somewhat unfortunate accusation. What is there in the hypothesis of *necessary*, as distinguished from *actual*, correlation of parts, which is particularly in harmony with Theism? The maintenance of the *necessity*, whether of sequences or of coexistences, is commonly thought rather a derogation from divine power than otherwise. Cuvier says—"None of these parts can be changed without affecting the others; and consequently, each taken separately, indicates and gives all the rest:" that is to say, in the nature of things the correlation *could not* have been otherwise. On the contrary, Professor Huxley contends we have no warrant for asserting that the correlation *could not* have been otherwise; but have not a little reason for thinking that the same physiological ends might have been differently secured. The one doctrine limits the possibilities of creation; the other denies the implied limit. Which, then, is most open to the charge of covert Atheism?

On the other point to which we have referred, we lean to the opinion of Professor Owen. We agree with him in thinking that where a rational correlation (in the highest sense of the term) can be made out, it affords a better basis for deduction than an empirical correlation ascertained only by accumulated observations. Premising that by rational correlation we do not mean one in which we can trace, or think we can trace, a design, but one of which the negation is inconceivable (and this is the species of correlation which Cuvier's law implies); then we hold that our knowledge of the correlation is of a more certain kind than where it is simply inductive. We think that Professor Huxley, in his anxiety to avoid

the error of making Thought the measure of Things, does not sufficiently bear in mind the fact, that as our notion of necessity is determined by some absolute uniformity pervading all orders of our experiences, it follows that an organic correlation which cannot be conceived otherwise, is guaranteed by a much wider induction than one ascertained only by the observation of organisms. But the truth is, that there are scarcely any organic correlations of which the negation is inconceivable. If we find the skull, vertebræ, ribs, and phalanges of some land-animal as large as an elephant; we may indeed be certain that the legs of this animal were of considerable size—much larger than those of a rat: and our reason for conceiving this correlation as necessary, is that it is based, not upon our experiences of moving organisms alone, but upon all our mechanical experiences relative to masses and their supports. Not only, however, are there few physiological correlations really of this order, but there is danger in pursuing this line of reasoning, in consequence of the liability to include within the class of truly necessary correlations, those which are not such. For instance, there would seem to be a necessary correlation between the eye and the surface of the body. The function of the eye being vision, and light being needful for vision, it might be supposed absolutely requisite that every eye should be external. Nevertheless it is a fact that there are creatures, as the *Cirrhipœdia*, whose eyes (not very efficient ones, it may be) are deeply imbedded in the substance of the body. Again, a necessary correlation might be supposed to exist between the dimensions of the mammalian uterus and those of the pelvis. It would appear *à priori* an impossibility that in any species there should exist a well-developed uterus containing a full-

sized fœtus, and yet that the arch of the pelvis should be so small as not to allow the fœtus to pass. And were the only mammal having a very small pelvic arch a fossil one, it would have been inferred, on the Cuvierian method, that the fœtus must have been born in a rudimentary state; and that the uterus must have been proportionally small. But there happens to be a living mammal having a very small pelvic arch—the mole—which presents us with a fact that saves us from this erroneous inference. Anomalous as the fact is, the young of the mole are not born through the pelvic arch at all; but in front of it! Thus, granting that some quite *direct* physiological correlations may be necessary, we see that there is great risk of including among them some that are not such.

With regard to the great mass of the correlations, however, including all the *indirect* ones, we agree with Professor Huxley in denying that they are necessary; and we now propose to show this deductively. Let us begin with an analogy.

Whoever has been through an extensive iron-works, has seen a gigantic pair of shears worked by machinery, and used for cutting in two, bars of iron that are from time to time thrust between its blades. Supposing these blades to be the only visible parts of the apparatus, any one observing their movements (or rather the movement of one, for the other is commonly fixed), will see from the manner in which the angle increases and decreases, and from the curve described by the moving extremity, that there must be some centre of motion round which the action takes place—either a pivot or an external box equivalent to it. This may be regarded as a necessary correlation. Moreover, he might infer that beyond the centre of motion the moving blade was pro-

duced into a lever, to which the power was applied ; but as another arrangement is just possible, this could not be called anything more than a highly probable correlation. If now he went a step further and considered how the alternating movement was given to the lever, he would very likely conclude that it was given by a crank. But if he knew anything of machinery, he would know that it might possibly be given by an eccentric. Or again, he would know that the effect could be achieved by a cam. That is to say, he would see that there was no necessary correlation between the shears and the remoter parts of the apparatus. Take another case. The plate of a printing-press is required to move up and down to the extent of an inch or so ; and it is further requisite that it shall exert its greatest pressure when it reaches the extreme of its downward movement. If now any one will look over the stock of a printing-press maker, he will see half a dozen different mechanical arrangements by which these ends are achieved ; and any clever machinist would tell him that as many more might readily be invented. Further, he would learn from the same authority, that in proportion to the complexity of a machine, is the number of possible arrangements of its other parts which may be made without altering some one part. Should any objection be made to the analogy between a machine and an organism, it cannot be on the ground that the constituent parts of a machine are *less* rigorously correlated than those of an organism ; for the reverse is the case—they are *more* rigorously correlated. An organism will continue to act when it has lost one or two of its limbs, or when one of the lungs is gone ; but the abstraction of such important parts from either of the machines above described would immediately stop it. If, then, there is no necessary

correlation between the various parts of a machine, still less is there between those of an organism.

From a converse point of view the same truth is manifest. Bearing in mind the above analogy, it will be foreseen that an alteration in one part of an organism does not necessarily entail *some one specific set of alterations in the other parts.* Cuvier says—"None of these parts can be changed without affecting the others; and consequently, each taken separately, indicates and gives all the rest." The first of these propositions may pass, but the second, which is alleged to follow from it, is not true; for it implies that "all the rest" can be severally affected in only one way and degree, whereas they can be affected in many ways and degrees. To show this, we must again have recourse to a mechanical analogy.

If you set a brick on end and thrust it over, you can predict with certainty in what direction it will fall, and what attitude it will assume. If, again setting it up, you put another on the top of it, you can no longer foresee with accuracy the results of an overthrow; and on repeating the experiment, no matter how much care is taken to place them in the same positions, and to apply the same degree of force in the same direction, the effects will on no two occasions be exactly alike. And in proportion as the aggregation is complicated by the addition of new and unlike parts, will the results of any disturbance become more varied and incalculable. If, instead of bodies placed in this loose mechanical dependence, you take a group held in more permanent connection—say tied together by strings, as the bones are tied by muscles and ligaments—it will be equally manifest that a disturbing force applied to one part, will affect the others, not in a definite, but in an indefinite way; and that no second group could be made so per-

fectly like the first, that an equivalent disturbance would produce exactly the same results. The like truth is clearly and curiously illustrated in the case of locomotive engines. It is a fact familiar to all mechanical engineers and engine-drivers, that out of a number of engines built as accurately as possible to the same pattern, no two will act in just the same manner. Each will have its idiosyncrasies. The play of actions and reactions will so far differ, that under like conditions each will behave in a somewhat different way; and every driver has to learn the constitution of his own engine before he can work it to the greatest advantage. In organisms themselves this indefiniteness of mechanical reaction is clearly traceable. Two boys throwing stones will always more or less differ in their attitudes; as will two billiard-players, or two persons dealing out cards. The familiar fact that each individual has a characteristic gait, illustrates the point still better. The rhythmical motion of the leg is simple, and on the Cuvierian hypothesis should react upon the body in some uniform way. But in consequence of those slight diversities of structure which consist with identity of species, no two individuals make exactly similar movements either of the trunk or the arms: there is always a peculiarity recognizable by their friends.

When we pass to disturbing forces of non-mechanical kinds, the same truth becomes still more conspicuous. Expose several persons to a drenching storm; and while one will subsequently feel no appreciable inconvenience, another will have a cough, another a catarrh, another an attack of diarrhœa, another a fit of rheumatism. Vaccinate several children of the same age with the same quantity of virus applied to the same part, and the symptoms will not be quite alike in any of them, either

in kind or intensity; and in some cases the differences will be extreme. The quantity of alcohol which will send one man to sleep will render another unusually brilliant—will make this maudlin, and that irritable—will here excite feelings of kindness, and there feelings of enmity. Opium will produce either drowsiness or wakefulness: so will tobacco. And without further multiplying illustrations familiar to every one, we may repeat what we recently heard asserted by one of our most scientific physicians, that there is scarcely an influence to which the body is subjected, but what may, under different circumstances, produce quite opposite effects.

Now in all these cases—mechanical and other—some force is brought to bear primarily on one part of an organism, and secondarily on the rest; and according to the doctrine of Cuvier, the rest ought to be affected in quite specific ways. We find it by no means true that they are. The original change produced in one part. does not stand in necessary correlation with every one of the changes produced in the other parts; nor do these stand in necessary correlations with each other. The functional alteration which the disturbing force causes in the organ directly acted upon, does not involve some *particular set* of functional alterations in the other organs; but will be followed by one out of various sets And it is a manifest corollary, that any *structural alteration* which may eventually be produced in the one organ, will not be accompanied by *some particular set of structural alterations* in the other organs: there will be no necessary correlation of forms.

The flaw in Cuvier's principle lies in assuming too specific a mutual dependence between the several parts of an organism. It is doubtless true, as he says, that

"none of these parts can be changed without affecting the others." And were the members of a species *absolutely* alike in their minutest details, and always in *absolutely* the same constitutional state; then, a change in any part must in every case be followed by one specific set of changes in the rest. But the absence of this absolute similarity vitiates his inference. The fact that no two individuals are exactly alike either in structure or state, involves the fact that the changes produced by any disturbing force will not be alike, but may be totally unlike. Just as delicately poised scales may, when shaken, preponderate either way, in virtue of some quite inappreciable difference: so, the organic equilibrium in two creatures of the same kind may, by the same disturbance, be overthrown in opposite directions, in consequence of those minute unlikenesses which exist in every case. And having had the organic equilibrium thus overthrown in opposite directions, a persistence of the disturbing cause may produce in them quite different sets of permanent organic changes.

Thus Palæontology must depend upon the empirical method. Necessary correlation cannot be substantiated. A fossil species that was obliged to change its food or habits of life, did not of necessity undergo the particular set of modifications exhibited; but, under some slight change of predisposing causes—as of season or latitude —might have undergone some other set of modifications: the determining circumstance being one which, in the human sense, we call fortuitous.

We venture to think, then, that the deductive method greatly elucidates this vexed question in physiology; while at the same time our argument collaterally exhibits the limits within which the deductive method is applicable. For while we see that this extremely *general*

question may be satisfactorily dealt with deductively; the conclusion at which we have arrived, itself implies that the more *special* phenomena of organization cannot be so dealt with.

With a brevity necessitated by fast-diminishing space, we must draw attention to yet another method of investigating the general truths of physiology—a method to which physiology already owes one luminous idea, but which is not at present formally recognized as a method. We refer to the comparison of physiological phenomena with social phenomena.

The analogy between individual organisms and the social organism, is one that has in all ages forced itself on the attention of the observant. Though modern science does not countenance those crude ideas of this analogy which have been from time to time expressed since the days of the Greeks; yet it more and more tends to show that there *is* an analogy, and a very remarkable one. While it is becoming clear that there are no such special parallelisms between the constituent parts of a man and those of a nation as have been thought to exist; it is also becoming clear that the general principles of development and structure displayed in all organized bodies are displayed in societies also. The fundamental characteristic both of societies and of living creatures, is, that they consist of mutually-dependent parts; and it would seem that this involves a community of various other characteristics. Most men who have any acquaintance with the broad facts of both physiology and sociology, are beginning to recognize this correspondence not as a plausible fancy, but as a scientific truth. And we are strongly of opinion that it will by and by be seen to hold to an extent which few at present suspect.

Meanwhile, if any such correspondence exists, it is clear that Biology and Sociology will more or less interpret each other. Each affords its special facilities for inquiry. Relations of cause and effect clearly traceable in the social organism, may lead to the search for analogous ones in the individual organism; and may so elucidate what might else be inexplicable. Laws of growth and function disclosed by the pure physiologist, may occasionally give us the clue to certain social modifications otherwise difficult to understand. If they can do no more, the two sciences can at least exchange suggestions and confirmations; and this will be no small aid. The conception of "the physiological division of labour," which political economy has already supplied to Biology, is one of no small value. And the probability is that it has others to give.

In support of this opinion, we will now cite cases in which such aid is furnished. And, in the first place, let us see whether the facts of social organization do not afford additional support to some of the doctrines set forth in the foregoing parts of this article.

One of the positions we have endeavoured to establish is, that in animals the process of development is carried on, not by differentiations only, but by subordinate integrations. Now in the social organism we may see the same duality of process; and further, it is to be observed that the integrations are of the same three kinds. Thus we have integrations that arise from the simple growth of adjacent parts that perform like functions; as for instance, the coalescence of Manchester with its calico-weaving suburbs. We have other integrations that arise when, out of several places producing a particular commodity, one monopolizes more and more of the business, and leaves the rest to dwindle; as witness the

growth of the Yorkshire cloth-districts at the expense of those in the west of England; or the absorption by Staffordshire of the pottery-manufacture, and the consequent decay of the establishments that once flourished at Derby, and elsewhere. And we have those yet other integrations that result from the actual approximation of the similarly-occupied parts; whence result such facts as the concentration of publishers in Paternoster Row, of lawyers in the Temple and neighbourhood, of corn-merchants about Mark Lane, of civil engineers in Great George Street, of bankers in the centre of the city. Finding thus that in the evolution of social organisms, as in the evolution of individual organisms, there are integrations as well as differentiations, and moreover that these integrations are of the same three kinds; we have additional reason for considering these integrations as essential parts of the developmental process, needing to be included in its formula. And further, the circumstance that in the social organism these integrations are dependent on community of function, confirms the hypothesis that they are thus dependent in the individual organism.

Again, we endeavoured to show deductively, that the contrasts of parts first seen in all unfolding embryos, are consequent upon the contrasted circumstances to which such parts are exposed; that thus, adaptation of constitution to conditions is the principle which determines their primary changes; and that, possibly, if we include under the formula hereditarily-transmitted adaptations, all differentiations arising afterwards may be similarly determined. Well, we need not long contemplate the facts to see that the predominant social differentiations are brought about in an analogous way. As the members of an originally-homogeneous community

multiply and spread, the gradual separation into sections which simultaneously takes place, manifestly depends on differences of local circumstances. Those who happen to live near some place chosen, perhaps for its centrality, as one of periodical assemblage, become traders, and a town springs up; those who live dispersed continue to hunt or cultivate the earth; those who spread to the sea-shore fall into maritime occupations. And each of these classes undergoes modifications of nature fitting it to its function. Later in the process of social evolution these local adaptations are greatly multiplied. In virtue of differences of soil and climate, the rural inhabitants in different parts of the kingdom have their occupations partially specialized; and are respectively distinguished as chiefly producing cattle or sheep, or wheat, or oats, or hops, or cyder. People living where coal-fields are discovered become colliers; Cornishmen take to mining because Cornwall is metalliferous; and the iron-manufacture is the dominant industry where ironstone is plentiful. Liverpool has assumed the office of importing cotton, in consequence of its proximity to the district where cotton goods are made; and for analogous reasons Hull has become the chief port at which foreign wools are brought in. Even in the establishment of breweries, of dye-works, of slate quarries, of brick-yards, we may see the same truth. So that both in general and in detail these specializations of the social organism which characterize separate districts, primarily depend on local circumstances. Of the originally-similar units making up the social mass, different groups assume the different functions which their respective positions entail; and become adapted to their conditions. Thus, that which we concluded, *à priori*, was the leading cause of organic differentiations, we find, *à posteriori*, to be the

leading cause of social differentiations. Nay further, as we inferred that possibly the embryonic changes which are not thus directly caused, are caused by hereditarily-transmitted adaptations; so, we may actually see that in embryonic societies, such changes as are not due to direct adaptations are in the main traceable to adaptations originally undergone by the parent society. The colonies founded by distinct nations, while they similarly exhibit specializations caused in the way above described, diverge in so far as they take on, more or less, the organizations of the nations they sprung from. A French settlement does not develop exactly after the same manner as an English one; and both assume forms unlike those which Roman settlements assumed. Now the fact that the differentiations of societies is determined partly by the direct adaptation of their units to local conditions, and partly by the transmitted influence of like adaptations undergone by ancestral societies, enforces the conclusion otherwise reached, that the differentiation of individual organisms results from immediate adaptations compounded with ancestral adaptations.

From confirmations thus furnished by Sociology to Physiology, let us now pass to a suggestion similarly furnished. A factory, or other producing establishment, or a town made up of such establishments, is an agency for elaborating some commodity consumed by society at large; and may be regarded as analogous to a gland or viscus in an individual organism. If now we inquire what is the primitive mode in which one of these producing establishments grows up, we find it to be this. A single worker, who himself sells the produce of his labour, is the germ. His business increasing, he employs helpers —sons or apprentices; and having done this, he becomes

a vendor not only of his own handiwork, but of that of others. A further increase of his business compels him to multiply his assistants, and his sale grows so rapid that he is obliged to confine himself to the process of selling; that is, he ceases to be a producer, and becomes simply a channel through which the produce of others is conveyed to the public. Should his prosperity rise yet higher, he finds that he is unable to manage even the sale of his commodities, and has to employ others, probably of his own family, to aid him in selling; that is, to him as a main channel are now added subordinate channels; and so on continuously. Moreover, when there grow up in one place, as a Manchester or a Birmingham, many establishments of like kind, this process is carried still further. There arise factors and buyers, who are the channels through which are transmitted the produce of many factories; and we believe that primarily these factors were manufacturers who undertook to dispose of the produce of smaller houses as well as their own, and ultimately became salesmen only. Under a converse aspect, all the stages of this development have been within these few years clearly exemplified in our railway contractors. There are sundry men now living who illustrate the whole process in their own persons—men who were originally navvies, digging and wheeling; who then undertook some small sub-contract, and worked along with those they paid; who presently took larger contracts and employed foremen; and who now contract for whole railways and let portions to subcontractors. That is to say, we have men who were originally workers, but have finally become the main channels out of which diverge secondary channels, which again bifurcate into the subordinate channels, through which flows the money (that is, the nutriment) supplied

by society to the actual makers of the railway. Now it seems worth inquiring whether this is not the original course followed in the evolution of secreting and excreting organs in an animal. We know that such organs begin as clusters of separate cells; are then seen as groups of follicles each containing many cells; and end as masses of such groups, permeated by ducts. But while analogy suggests that this is the *original* mode in which such organs are developed, it suggests that this does not continue to be the mode. For as we find that in the social organism, manufacturing establishments are no longer commonly developed through the series of modifications above described, but now mostly arise by the direct transformation of a number of persons into master, clerks, foremen, workers, &c.; so the approximative method of forming organs, may in some cases be replaced by a direct metamorphosis of the organic elements into the destined structure without any transitional structures being passed through. That there are organs thus formed is an ascertained fact; and the additional question which analogy suggests is, whether the direct method is substituted for the indirect method.

These few illustrations will sufficiently justify our opinion that the study of organized bodies may be indirectly furthered by the study of the body politic: hints, at least, may be expected, if nothing more. The Inductive Method, usually alone employed by most physiologists, may not only derive important assistance from the Deductive Method, but it may further be supplemented by the Sociological Method.

XIII.

THE COMPARATIVE PSYCHOLOGY OF MAN.

[READ BEFORE THE ANTHROPOLOGICAL INSTITUTE, JUNE 22, 1875.]

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THE COMPARATIVE PSYCHOLOGY OF MAN.

WHILE discussing with two members of the Anthropological Institute the work to be undertaken by its psychological section, I made certain suggestions which they requested me to put in writing. When reminded, some months after, of the promise I had made to do this, I failed to recall the particular suggestions referred to; but in the endeavour to remember them, I was led to glance over the whole subject of comparative human psychology. Hence resulted the following paper.

That making a general survey is useful as a preliminary to deliberate study, either of a whole or of any part, scarcely needs showing. Vagueness of thought accompanies the wandering about in a region without known bounds or landmarks. Attention devoted to some portion of a subject in ignorance of its connexion with the rest, leads to untrue conceptions. The whole cannot be rightly conceived without some knowledge of the parts; and no part can be rightly conceived out of relation to the whole.

To map out the Comparative Psychology of Man must also conduce to the more methodic carrying on of inquiries. In this, as in other things, division of labour will facilitate progress; and that there may be division of labour, the work itself must be systematically divided.

We may conveniently separate the entire subject into three main divisions, arranged in the order of increasing speciality.

The first division will treat of the degrees of mental evolution of different human types, generally considered: taking account of both the mass of mental manifestation and the complexity of mental manifestation. This division will include the relations of these characters to physical characters—the bodily mass and structure, and the cerebral mass and structure. It will also include inquiries concerning the time taken in completing mental evolution, and the time during which adult mental power lasts; as well as certain most general traits of mental action, such as the greater or less persistence of emotions and of intellectual processes. The connexion between the general mental type and the general social type should also be here dealt with.

In the second division may be conveniently placed apart, inquiries concerning the relative mental natures of the sexes in each race. Under it will come such questions as these:—What differences of mental mass and mental complexity, if any, existing between males and females, are common to all races? Do such differences vary in degree, or in kind, or in both? Are there reasons for thinking that they are liable to change by increase or decrease? What relations do they bear in each case to the habits of life, the domestic arrangements, and the social arrangements? This division should also include in its scope the sentiments of the sexes towards one another, considered as varying quantitatively and qualitatively; as well as their respective sentiments towards offspring, similarly varying.

For the third division of inquiries may be reserved the more special mental traits distinguishing different

types of men. One class of such specialities results from differences of proportion among faculties possessed in common; and another class results from the presence in some races of faculties that are almost or quite absent from others. Each difference in each of these groups, when established by comparison, has to be studied in connexion with the stage of mental evolution reached, and has to be studied in connexion with the habits of life and the social development, regarding it as related to these both as cause and consequence.

Such being the outlines of these several divisions, let us now consider in detail the subdivisions contained within each.

I.—Under the head of general mental evolution we may begin with the trait of—

1. *Mental mass.*—Daily experiences show us that human beings differ in volume of mental manifestation. Some there are whose intelligence, high though it may be, produces little impression on those around; while there are some who, when uttering even commonplaces, do it so as to affect listeners in a disproportionate degree. Comparison of two such makes it manifest that, generally, the difference is due to the natural language of the emotions. Behind the intellectual quickness of the one there is not felt any power of character; while the other betrays a momentum capable of bearing down opposition—a potentiality of emotion that has something formidable about it. Obviously the varieties of mankind differ much in respect of this trait. Apart from kind of feeling, they are unlike in amount of feeling. The dominant races overrun the inferior races mainly in virtue of the greater quantity of energy in which this greater mental mass shows itself. Hence a series of in-

quiries, of which these are some :—(*a*) What is the relation between mental mass and bodily mass? Manifestly, the small races are deficient in it. But it also appears that races much upon a par in size—as, for instance, an Englishman and a Damara, differ considerably in mental mass. (*b*) What is its relation to mass of brain? and, bearing in mind the general law that in the same species, size of brain increases with size of body (though not in the same proportion), how far can we connect the extra mental mass of the higher races, with an extra mass of brain beyond that which is proper to their greater bodily mass? (*c*) What relation, if any, is there between mental mass and the physiological state expressed in vigour of circulation and richness of blood, as severally determined by mode of life and general nutrition? (*d*) What are the relations of this trait to the social state, as nomadic or settled, predatory or industrial?

2. *Mental complexity.*—How races differ in respect of the more or less involved structures of their minds, will best be understood on recalling that unlikeness between the juvenile mind and the adult mind among ourselves, which so well typifies the unlikeness between the minds of savage and civilized. In the child we see absorption in special facts. Generalities even of a low order are scarcely recognized, and there is no recognition of high generalities. We see interest in individuals, in personal adventures, in domestic affairs, but no interest in political or social matters. We see vanity about clothes and small achievements, but little sense of justice: witness the forcible appropriation of one another's toys. While there have come into play many of the simpler mental powers, there has not yet been reached that complication of mind which results from the addition of powers evolved out of these simpler ones. Kindred differences

of complexity exist between the minds of lower and higher races; and comparisons should be made to ascertain their kinds and amounts. Here, too, there may be a subdivision of the inquiries. (*a*) What is the relation between mental complexity and mental mass? Do not the two habitually vary together? (*b*) What is the relation to the social state, as more or less complex? that is to say—Do not mental complexity and social complexity act and react on each other?

3. *Rate of mental development.*—In conformity with the biological law that the higher the organisms the longer they take to evolve, members of the inferior human races may be expected to complete their mental evolution sooner than members of the superior races; and we have evidence that they do this. Travellers from all regions comment, now on the great precocity of children among savage and semi-civilized peoples, and now on the early arrest of their mental progress. Though we scarcely need more proofs that this general contrast exists, there remains to be asked the question, whether it is consistently maintained throughout all orders of races, from the lowest to the highest—whether, say, the Australian differs in this respect from the Hindu, as much as the Hindu does from the European. Of secondary inquiries coming under this sub-head may be named several. (*a*) Is this more rapid evolution and earlier arrest always unequally shown by the two sexes; or, in other words, are there in lower types proportional differences in rate and degree of development, such as higher types show us? (*b*) Is there in many cases, as there appears to be in some cases, a traceable relation between the period of arrest and the period of puberty? (*c*) Is mental decay early in proportion as mental evolution is rapid? (*d*) Can we in other respects assert that where

the type is low, the entire cycle of mental changes between birth and death—ascending, uniform, descending—comes within a shorter interval?

4. *Relative plasticity.*—Is there any relation between the degree of mental modifiability which remains in adult life, and the character of the mental evolution in respect of mass, complexity, and rapidity? The animal kingdom at large yields us reasons for associating an inferior and more rapidly-completed mental type, with a relatively automatic nature. Lowly organized creatures, guided almost entirely by reflex actions, are in but small degrees changeable by individual experiences. As the nervous structure complicates, its actions become less rigorously confined within pre-established limits; and as we approach the highest creatures, individual experiences take larger and larger shares in moulding the conduct: there is an increasing ability to take in new impressions and to profit by the acquisitions. Inferior and superior human races are contrasted in this respect. Many travellers comment on the unchangeable habits of savages. The semi-civilized nations of the East, past and present, were, or are, characterized by a greater rigidity of custom than characterizes the more civilized nations of the West. The histories of the most civilized nations show us that in their earlier times, the modifiability of ideas and habits was less than it is at present. And if we contrast classes or individuals around us, we see that the most developed in mind are the most plastic. To inquiries respecting this trait of comparative plasticity, in its relations to precocity and early completion of mental development, may fitly be added inquiries respecting its relations to the social state, which it helps to determine, and which reacts upon it.

5. *Variability.*—To say of a mental nature that its

actions are extremely inconstant, and at the same time to say that it is a relatively unchangeable nature, apparently implies a contradiction. When, however, the inconstancy is understood as referring to the manifestations which follow one another from minute to minute, and the unchangeableness to the average manifestations, extending over long periods, the apparent contradiction disappears; and it becomes comprehensible that the two traits may, and ordinarily do, co-exist. An infant, quickly weary with each kind of perception, wanting ever a new object which it soon abandons for something else, and alternating a score times a day between smiles and tears, shows us a very small persistence in each kind of mental action: all its states, intellectual and emotional, are transient. Yet at the same time its mind cannot be easily changed in character. True, it changes spontaneously in due course; but it long remains incapable of receiving ideas or emotions beyond those of simple orders. The child exhibits less rapid variations, intellectual and emotional, while its educability is greater. Inferior human races show us this combination: great rigidity of general character with great irregularity in its passing manifestations. Speaking broadly, while they resist permanent modification they lack intellectual persistence, and they lack emotional persistence. Of various low types we read that they cannot keep the attention fixed beyond a few minutes on anything requiring thought, even of a simple kind. Similarly with their feelings: these are less enduring than those of civilized men. There are, however, qualifications to be made in this statement; and comparisons are needed to ascertain how far these qualifications go. The savage shows great persistence in the action of the lower intellectual faculties. He is untiring in minute observation. He is un-

tiring, also, in that kind of perceptive activity which accompanies the making of his weapons and ornaments: often persevering for immense periods in carving stones, &c. Emotionally, too, he shows persistence not only in the motives prompting these small industries, but also in certain of his passions—especially in that of revenge. Hence, in studying the degrees of mental variability shown us in the daily lives of the different races, we must ask how far variability characterizes the whole mind, and how far it holds only of parts of the mind.

6. *Impulsiveness.*—This trait is closely allied with the last: unenduring emotions are emotions which sway the conduct now this way and now that, without any consistency. The trait of impulsiveness may, however, be fitly dealt with separately, because it has other implications than mere lack of persistence. Comparisons of the lower human races with the higher, appear generally to show that, along with brevity of the passions, there goes violence. The sudden gusts of feeling which men of inferior types display, are excessive in degree as they are short in duration; and there is probably a connexion between these two traits: intensity sooner producing exhaustion. Observing that the passions of childhood illustrate this connexion, let us turn to certain interesting questions concerning the decrease of impulsiveness which accompanies advance in evolution. The nervous processes of an impulsive being, are less remote from reflex actions than are those of an unimpulsive being. In reflex actions we see a simple stimulus passing suddenly into movement: little or no control being exercised by other parts of the nervous system. As we ascend to higher actions, guided by more and more complicated combinations of stimuli, there is not the same instantaneous discharge in simple motions; but there is

a comparatively deliberate and more variable adjustment of compound motions, duly restrained and proportioned. It is thus with the passions and sentiments in the less developed natures and in the more developed natures. Where there is but little emotional complexity, an emotion, when excited by some occurrence, explodes in action before the other emotions have been called into play; and each of these, from time to time, does the like. But the more complex emotional structure is one in which these simpler emotions are so co-ordinated that they do not act independently. Before excitement of any one has had time to cause action, some excitement has been communicated to others—often antagonistic ones—and the conduct becomes modified in adjustment to the combined dictates. Hence results a decreased impulsiveness, and also a greater persistence. The conduct pursued, being prompted by several emotions co-operating in degrees which do not exhaust them, acquires a greater continuity; and while spasmodic force becomes less conspicuous, there is an increase in the total energy. Examining the facts from this point of view, there are sundry questions of interest to be put respecting the different races of men. (*a*) To what other traits than degree of mental evolution is impulsiveness related? Apart from difference in elevation of type, the New-World races seem to be less impulsive than the Old-World races. Is this due to constitutional apathy? Can there be traced (other things equal) a relation between physical vivacity and mental impulsiveness? (*b*) What connexion is there between this trait and the social state? Clearly a very explosive nature—such as that of the Bushman—is unfit for social union; and, commonly, social union, when by any means established, checks impulsiveness. (*c*) What respective shares in checking

impulsiveness are taken by the feelings which the social state fosters—such as the fear of surrounding individuals, the instinct of sociality, the desire to accumulate property, the sympathetic feelings, the sentiment of justice? These which require a social environment for their development, all of them involve imaginations of consequences more or less distant; and thus imply checks upon the promptings of the simpler passions. Hence arise the questions—In what order, in what degrees, and in what combinations, do they come into play?

7. One further general inquiry of a different kind may be added. What effect is produced on mental nature by mixture of races? There is reason for believing that throughout the animal kingdom, the union of varieties that have become widely divergent is physically injurious; while the union of slightly divergent varieties is physically beneficial. Does the like hold with the mental nature? Some facts seem to show that mixture of human races extremely unlike produces a worthless type of mind—a mind fitted neither for the kind of life led by the higher of the two races, nor for that led by the lower—a mind out of adjustment to all conditions of life. Contrariwise, we find that peoples of the same stock, slightly differentiated by lives carried on in unlike circumstances for many generations, produce by mixture a mental type having certain superiorities. In his work on *The Huguenots*, Mr. Smiles points out how large a number of distinguished men among us have descended from Flemish and French refugees; and M. Alphonse De Candolle, in his *Histoire des Sciences et des Savants depuis deux Siècles*, shows that the descendants of French refugees in Switzerland have produced an unusually great proportion of scientific men. Though, in

part, this result may be ascribed to the original natures of such refugees, who must have had that independence which is a chief factor in originality, yet it is probably in part due to mixture of races. For thinking this, we have evidence which is not open to two interpretations. Prof. Morley draws attention to the fact that, during seven hundred years of our early history, "the best genius of England sprang up on the line of country in which Celts and Anglo-Saxons came together." In like manner, Mr. Galtōn, in his *English Men of Science*, shows that in recent days these have mostly come from an inland region, running generally from north to south, which we may reasonably presume contains more mixed blood than do the regions east and west of it. Such a result seems probable *à priori*. Two natures respectively adapted to slightly unlike sets of social conditions, may be expected by their union to produce a nature somewhat more plastic than either—a nature more impressible by the new circumstances of advancing social life, and therefore more likely to originate new ideas and display modified sentiments. The comparative psychology of man may, then, fitly include the mental effects of mixture; and among derivative inquiries we may ask—How far the conquest of race by race has been instrumental in advancing civilization by aiding mixture, as well as in other ways?

II.—The second of the three leading divisions named at the outset is less extensive. Still, concerning the relative mental natures of the sexes in each race, questions of much interest and importance may be raised.

1. *Degree of difference between the sexes.*—It is an established fact that, physically considered, the contrast between males and females is not equally great in all

types of mankind. The bearded races, for instance, show us a greater unlikeness between the two than do the beardless races. Among South American tribes, men and women have a greater general resemblance in form, &c., than is usual elsewhere. The question, then, suggests itself—Do the mental natures of the sexes differ in a constant or in a variable degree? The difference is unlikely to be a constant one; and, looking for variation, we may ask what is its amount, and under what conditions does it occur?

2. *Difference in mass and in complexity.*—The comparisons between the sexes, of course, admit of subdivisions parallel to those made in the comparisons between the races. Relative mental mass and relative mental complexity have chiefly to be observed. Assuming that the great inequality in the cost of reproduction to the two sexes is the cause of unlikeness in mental mass, as in physical mass, this difference may be studied in connexion with reproductive differences presented by the various races, in respect of the ages at which reproduction commences, and the periods over which it lasts. An allied inquiry may be joined with this; namely, how far the mental developments of the two sexes are affected by their relative habits in respect to food and physical exertion? In many of the lower races, the women, treated with great brutality, are, physically, much inferior to the men: excess of labour and defect of nutrition being apparently the combined causes. Is any arrest of mental development simultaneously caused?

3. *Variation of the differences.*—If the unlikeness, physical and mental, of the sexes is not constant, then, supposing all races have diverged from one original stock, it follows that there must have been transmission of accumulated differences to those of the same sex in

posterity. If, for instance, the prehistoric type of man was beardless, then the production of a bearded variety implies that within that variety the males continued to transmit an increasing amount of beard to descendants of the same sex. This limitation of heredity by sex, shown us in multitudinous ways throughout the animal kingdom, probably applies to the cerebral structures as much as to other structures. Hence the question—Do not the mental natures of the sexes in alien types of Man diverge in unlike ways and degrees?

4. *Causes of the differences.*—Are any relations to be traced between these variable differences and the variable parts the sexes play in the business of life? Assuming the cumulative effects of habit on function and structure, as well as the limitation of heredity by sex, it is to be expected that if, in any society, the activities of one sex, generation after generation, differ from those of the other, there will arise sexual adaptations of mind. Some instances in illustration may be named. Among the Africans of Loango and other districts, as also among some of the Indian Hill-tribes, the men and women are strongly contrasted as respectively inert and energetic: the industry of the women having apparently become so natural to them that no coercion is needed. Of course, such facts suggest an extensive series of questions. Limitation of heredity by sex may account both for those sexual differences of mind which distinguish men and women in all races, and for those which distinguish them in each race, or each society. An interesting subordinate inquiry may be, how far such mental differences are inverted in cases where there is inversion of social and domestic relations; as among those Khasi Hill-tribes, whose women have so far the upper hand that they turn off their husbands in a summary way if they displease them.

5. *Mental modifiability in the two sexes.*—Along with comparisons of races in respect of mental plasticity may go parallel comparisons of the sexes in each race. Is it true always, as it appears to be generally true, that women are less modifiable than men? The relative conservatism of women—their greater adhesion to established ideas and practices—is manifest in many civilized and semi-civilized societies. Is it so among the uncivilized? A curious instance of stronger attachment to custom in women than in men is given by Dalton, as occurring among the Juangs, one of the lowest wild tribes of Bengal. Until recently the only dress of both sexes was something less than that which the Hebrew legend gives to Adam and Eve. Years ago the men were led to adopt a cloth bandage round the loins, in place of the bunch of leaves; but the women adhered to the aboriginal habit: a conservatism shown where it might have been least expected.

6. *The sexual sentiment.*—Results of value may be looked for from comparisons of races made to determine the amounts and characters of the higher feelings to which the relation of the sexes gives rise. The lowest varieties of mankind have but small endowments of these feelings. Among varieties of higher types, such as the Malayo-Polynesians, these feelings seem considerably developed: the Dyaks, for instance, sometimes display them in great strength. Speaking generally, they appear to become stronger with the advance of civilization. Several subordinate inquiries may be named. (*a*) How far is development of the sexual sentiment dependent upon intellectual advance—upon growth of imaginative power? (*b*) How far is it related to emotional advance; and especially to evolution of those emotions which originate from sympathy? What are its relations to

polyandry and polygyny? (*c*) Does it not tend towards, and is it not fostered by, monogamy? (*d*) What connexion has it with maintenance of the family bond, and the consequent better rearing of children?

III.—Under the third head, to which we may now pass, come the more special traits of the different races.

1. *Imitativeness.*—One of the characteristics in which the lower types of men show us a smaller departure from reflex action than do the higher types, is their strong tendency to mimic the motions and sounds made by others—an almost involuntary habit which travellers find it difficult to check. This meaningless repetition, which seems to imply that the idea of an observed action cannot be framed in the mind of the observer without tending forthwith to discharge itself in the action conceived (and every ideal action is a nascent form of the consciousness accompanying performance of such action), evidently diverges but little from the automatic; and decrease of it is to be expected along with increase of self-regulating power. This trait of automatic mimicry is evidently allied with that less automatic mimicry which shows itself in greater persistence of customs. For customs adopted by each generation from the last without thought or inquiry, imply a tendency to imitate which overmasters critical and sceptical tendencies: so maintaining habits for which no reasons can be given. The decrease of this irrational mimicry, strongest in the lowest savage and feeblest in the highest of the civilized, should be studied along with the successively higher stages of social life, as being at once an aid and a hindrance to civilization: an aid in so far as it gives that fixity to the social organization without which a society cannot survive; a hindrance in so far as it offers resist-

ance to changes of social organization that have become desirable.

2. *Incuriosity.*—Projecting our own natures into the circumstances of the savage, we imagine ourselves as marvelling greatly on first seeing the products and appliances of civilized life. But we err in supposing that the savage has feelings such as we should have in his place. Want of rational curiosity respecting these incomprehensible novelties, is a trait remarked of the lowest races wherever found; and the partially-civilized races are distinguished from them as exhibiting rational curiosity. The relation of this trait to the intellectual nature, to the emotional nature, and to the social state, should be studied.

3. *Quality of thought.*—Under this vague head may be placed many sets of inquiries, each of them extensive—(*a*) The degree of generality of the ideas; (*b*) the degree of abstractness of the ideas; (*c*) the degree of definiteness of the ideas; (*d*) the degree of coherence of the ideas; (*e*) the extent to which there have been developed such notions as those of *class*, of *cause*, of *uniformity*, of *law*, of *truth*. Many conceptions which have become so familiar to us that we assume them to be the common property of all minds, are no more possessed by the lowest savages than they are by our own children; and comparisons of types should be so made as to elucidate the processes by which such conceptions are reached. The development under each head has to be observed—(*a*) independently in its successive stages; (*b*) in connexion with the co-operative intellectual conceptions; (*c*) in connexion with the progress of language, of the arts, and of social organization. Already linguistic phenomena have been used in aid of such inquiries; and more systematic use of them should be made. Not

only the number of general words, and the number of abstract words, in a people's vocabulary should be taken as evidence, but also their *degrees* of generality and abstractness; for there are generalities of the first, second, third, &c., orders, and abstractions similarly ascending in degree. *Blue* is an abstraction referring to one class of impressions derived from visible objects; *colour* is a higher abstraction referring to many such classes of visual impressions; *property* is a still higher abstraction referring to classes of impressions received not through the eyes alone, but through other sense-organs. If generalities and abstractions were arranged in the order of their extensiveness and in their grades, tests would be obtained which, applied to the vocabularies of the uncivilized, would yield definite evidence of the intellectual stages reached.

4. *Peculiar aptitudes.*—To such specialities of intelligence as mark different degrees of evolution, have to be added minor ones related to modes of life: the kinds and degrees of faculty which have become organized in adaptation to daily habits—skill in the use of weapons, powers of tracking, quick discrimination of individual objects. And under this head may fitly come inquiries concerning some race-peculiarities of the æsthetic class, not at present explicable. While the remains from the Dordogne caves show us that their inhabitants, low as we must suppose them to have been, could represent animals, both by drawing and carving, with some degree of fidelity; there are existing races, probably higher in other respects, who seem scarçely capable of recognizing pictorial representations. Similarly with the musical faculty. Almost or quite wanting in some inferior races, we find it in other races not of high grade, developed to an unexpected degree: instance the Negroes, some of

whom are so innately musical, that, as I have been told by a missionary among them, the children in native schools when taught European psalm-tunes, spontaneously sing seconds to them. Whether any causes can be discovered for race-peculiarities of this kind, is a question of interest.

5. *Specialities of emotional nature.*—These are worthy of careful study, as being intimately related to social phenomena—to the possibility of social progress, and to the nature of the social structure. Among others to be noted there are—(*a*) Gregariousness or sociality —a trait in the strength of which races differ widely: some, as the Mantras, being almost indifferent to social intercourse; some being unable to dispense with it. Obviously the degree of this desire for the presence of fellow-men, affects greatly the formation of social groups, and consequently influences social progress. (*b*) Intolerance of restraint. Men of some inferior types, as the Mapuché, are ungovernable; while those of other types, no higher in grade, not only submit to restraint, but admire the persons exercising it. These contrasted natures have to be observed in connexion with social evolution; to the early stages of which they are respectively antagonistic and favourable. (*c*) The desire for praise is a trait which, common to all races, high and low, varies considerably in degree. There are quite inferior races, as some of those in the Pacific States, whose members sacrifice without stint to gain the applause which lavish generosity brings; while, elsewhere, applause is sought with less eagerness. Notice should be taken of the connexion between this love of approbation and the social restraints; since it plays an important part in the maintenance of them. (*d*) The acquisitive propensity. This, too, is a character the degrees of

which, and the relations of which to the social state, have to be especially noted. The desire for property grows along with the possibility of gratifying it; and this, extremely small among the lowest men, increases as social development goes on. With the advance from tribal property to family property and individual property, the notion of private right of possession gains definiteness, and the love of acquisition strengthens. Each step towards an orderly social state makes larger accumulations possible, and the pleasures achievable by them more sure; while the resulting encouragement to accumulate, leads to increase of capital and further progress. This action and re-action of the sentiment and the social state, should be in every case observed.

6. *The altruistic sentiments.*—Coming last, these are also highest. The evolution of them in the course of civilization, shows us clearly the reciprocal influences of the social unit and the social organism. On the one hand, there can be no sympathy, nor any of the sentiments which sympathy generates, unless there are fellow-beings around. On the other hand, maintenance of union with fellow-beings depends in part on the presence of sympathy, and the resulting restraints on conduct. Gregariousness or sociality favours the growth of sympathy; increased sympathy conduces to closer sociality and a more stable social state; and so, continuously, each increment of the one makes possible a further increment of the other. Comparisons of the altruistic sentiments resulting from sympathy, as exhibited in different types of men and different social states, may be conveniently arranged under three heads—(*a*) Pity, which should be observed as displayed towards offspring, towards the sick and aged, and towards enemies. (*b*) Generosity (duly discriminated from the love of display)

as shown in giving; as shown in the relinquishment of pleasures for the sake of others; as shown by active efforts on others' behalf. The manifestations of this sentiment, too, are to be noted in respect of their range —whether they are limited to relatives; whether they extend only to those of the same society; whether they extend to those of other societies; and they are also to be noted in connexion with the degree of providence— whether they result from sudden impulses obeyed without counting the cost, or go along with clear foresight of the future sacrifices entailed. (c) Justice. This most abstract of the altruistic sentiments is to be considered under aspects like those just named, as well as under many other aspects—how far it is shown in regard to the lives of others; how far in regard to their freedom; how far in regard to their property; how far in regard to their various minor claims. And comparisons concerning this highest sentiments hould, beyond all others, be carried on along with comparisons of the accompanying social states, which it largely determines—the forms and actions of governments; the characters of the laws; the relations of classes.

Such, stated as briefly as consists with clearness, are the leading divisions and subdivisions under which the Comparative Psychology of Man may be arranged. In going rapidly over so wide a field, I have doubtless overlooked much that should be included. Doubtless, too, various of the inquiries named will branch out into subordinate inquiries well worth pursuing. Even as it is, however, the programme is extensive enough to occupy numerous investigators, who may with advantage take separate divisions.

Though, after occupying themselves with primitive

arts and products, anthropologists have devoted their attention mainly to the physical characters of the human races; it must, I think, be admitted that the study of these yields in importance to the study of their psychical characters. The general conclusions to which the first set of inquiries may lead, cannot so much affect our views respecting the highest classes of phenomena as can the general conclusions to which the second set may lead. A true theory of the human mind vitally concerns us; and systematic comparisons of human minds, differing in their kinds and grades, will help us in forming a true theory. Knowledge of the reciprocal relations between the characters of men and the characters of the societies they form, must influence profoundly our ideas of political arrangements. When the interdependence of individual natures and social structures is understood, our conceptions of the changes now taking place, and hereafter to take place, will be rectified. A comprehension of mental development as a process of adaptation to social conditions, which are continually remoulding the mind and are again remoulded by it, will conduce to a salutary consciousness of the remoter effects produced by institutions upon character; and will check the grave mischiefs which ignorant legislation now causes. Lastly, a right theory of mental evolution as exhibited by humanity at large, giving a key, as it does, to the evolution of the individual mind, must help to rationalize our perverse methods of education; and so to raise intellectual power and moral nature.